NOTICES

OF THE

PROCEEDINGS

AT THE

MEETINGS OF THE MEMBERS

OF THE

Royal Institution of Great Britain,

WITH

ABSTRACTS OF THE DISCOURSES

DELIVERED AT

THE EVENING MEETINGS.

VOLUME X.

1882—1884.

LONDON:

PRINTED BY WILLIAM CLOWES AND SONS, LIMITED,

STAMFORD STREET AND CHARING CROSS.

1884.

Patron.

HER MOST GRACIOUS MAJESTY

QUEEN VICTORIA.

Vice-Patron and Honorary Member.

HIS ROYAL HIGHNESS

THE PRINCE OF WALES, K.G. F.R.S.

President—THE DUKE OF NORTHUMBERLAND, D.C.L. LL.D.

Treasurer—GEORGE BUSK, Esq. F.R.S.—*V.P.*

Honorary Secretary—SIR WILLIAM BOWMAN, Bart. LL.D. F.R.S.—*V.P.*

MANAGERS. 1884–85.

George Berkley, Esq. M.I.C.E.
Sir Frederick J. Bramwell, F.R.S.—*V.P.*
Joseph Brown, Esq. Q.C.
Warren De La Rue, Esq. M.A. D.C.L. F.R.S.—*V.P.*
Captain Douglas Galton, C.B. D.C.L. F.R.S.
Colonel James Augustus Grant, C.B. C.S.I. F.R.S.
The Hon. Sir Wm. Robt. Grove, M.A. D.C.L. F.R.S.—*V.P.*
Right Hon. The Lord Claud Hamilton, J.P. (*deceased*).
Sir John Hawkshaw, F.R.S. F.G.S.
Sir John Lubbock, Bart. M.P. D.C.L. LL.D. F.R.S.—*V.P.*
Hugo W. Müller, Esq. Ph.D. F.R.S.
Sir Frederick Pollock, Bart. M.A.—*V.P.*
John Rae, M.D. LL.D. F.R.S.
The Earl of Rosse, D.C.L. LL.D. F.R.S.—*V.P.*
The Hon. Rollo Russell, F.M.S.

VISITORS. 1884–85.

John Birkett, Esq. F.L.S. F.R.C.S.
Charles James Busk, Esq.
Stephen Busk, Esq.
George Frederick Chambers, Esq. F.R.A.S.
William Crookes, Esq. F.R.S.
Rear-Admiral Herbert P. De Kantzow, R.N.
William Henry Domville, Esq.
Alexander John Ellis, Esq. B.A. F.S.A. F.R.S.
Rev. John Macnaught, M.A.
Robert James Mann, M.D. F.R.C.S.
Sir Thomas Pycroft, M.A. K.C.S.I.
Lachlan Mackintosh Rate, Esq. M.A.
John Bell Sedgwick, Esq. F.R.G.S.
Basil Woodd Smith, Esq. F.R.A.S.
Charles Meymott Tidy, Esq. M.B. F.C.S.

Professors.

Professor of Natural Philosophy—JOHN TYNDALL, Esq. D.C.L. LL.D. F.R.S. &c.

Fullerian Professor of Chemistry—JAMES DEWAR, Esq. M.A. F.R.S. Jacksonian Professor of Natural Experimental Philosophy, Univ. Cambridge.

Fullerian Professor of Physiology—Vacant.

Keeper of the Library and Assistant Secretary—Mr. Benjamin Vincent.

Assistant in the Library—Mr. Henry Young.

Clerk of Accounts and Collector—Mr. Henry C. Hughes.

Assistant in the Physical Laboratory—Mr. John Cottrell.

Assistant in the Chemical Laboratory—Mr. R. N. Lennox.

CONTENTS.

1881.

1882.

1883.

PLATES.

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 20, 1882.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

William Huggins, Esq. D.C.L. LL.D. F.R.S.

On Comets.

In the olden time comets were looked upon as the portents of all kinds of woe. In the words of Du Bartas, as rendered by Sylvester* :—

> " There with long bloudy haire, a Blazing Star
> Threatens the World with Famin, Plague and War:
> To Princes, death: to Kingdoms, many crosses:
> To all Estates, ineuitable Losses:
> To Heard-men, Rot: to Plough-men, hap-lesse Seasons:
> To Saylers, Storms: to Cities, ciuill Treasons."

In the past year, including telescopic comets, no fewer than seven of these blazing stars have threatened us, and certain contemporary events might seem, indeed, to justify this view of their malign influence. But though comets are no longer a terror to us, they are still, in some respects, a great mystery. When we attempt to explain the marvellous phenomena they present, by the rigid application of the laws of physics, we find ourselves confronted by prodigious difficulties. We are almost led to think that in the heavens, at least, there is more " than is dreamt of in our philosophy "—some profound and still unknown mystery of nature. Not to mention the many absurd theories which are heard on all sides when a great comet appears, on no phenomenon of nature have we so many guesses at truth by the masters of science on different and even opposing principles of explanation. At the present moment there is no consensus of opinion as to the nature of comets.

But within the last few years, from two opposite directions—from the use of the spectroscope and from mathematical investigation applied to the periodical displays of shooting stars—much knowledge has been gained of their nature, though there are still many points on which we can only speculate.

It is my purpose this evening to give chief prominence to the new knowledge which these two methods of research have placed within our reach, and to distinguish as clearly as may be possible between what we know about comets and what is not more than speculation.

To carry out this purpose we must first study carefully the phe-

* Du Bartas, translated by J. Sylvester, fol. 1621, p. 33.

nomena which have to be explained, namely, the essential appearances and changes which comets present during their approach to the sun, at the time that they are visible to us.

It is not necessary here to describe in detail the more purely astronomical side of the subject. It will be sufficient to say, in two words, that some comets have become permanent members of our system, while others probably visit us once only, never to return. It depends upon a comet's velocity whether its course shall be a hyperbola, a parabola, or an ellipse. In the latter case only can it become a permanently attached member of our system. If the velocity of the comet when at the earth's distance from the sun exceeds 26 miles a second, the comet must go off into space, never to come back to us. If the comet is moving less swiftly its path will return into itself, and it will visit us periodically after longer or shorter wanderings. In the case of many comets, including the brightest comet of last year, their velocity is so near the parabolic limit that it is scarcely possible, from observations made in the small part of their orbit near the sun, to be quite sure whether they will return to us or not. A number of comets, chiefly small ones, are certainly periodic, and of some comets several returns, true to the calculated time, have been observed.

The small portion of the comet's life during which we are able to study it is quite unlike its ordinary humdrum existence. It consists of the short period of extreme excitement into which it is thrown by a more or less near approach to the sun—a state of things which is accompanied by rapid and marvellous changes, often on a stupendous scale.

The appearances which comets put on under the sun's influence differ widely from each other. A few of these forms, passing from an almost invisible nebulosity up to a brilliant comet of the grand type, are represented on these diagrams. In nearly all these forms three essentially distinctive parts may be seen.

1. *The nucleus.* With the aid of a telescope, in the heads of most comets a minute bright point may be found. This apparently insignificant speck is truly the heart and kernel of the whole thing—potentially it is the comet. It is this small part alone which conforms rigorously to the laws of gravitation, and moves strictly in its orbit. If we could see a great comet during its distant wanderings when it has put off the gala trappings of perihelion, it would be a very sober object, and consist of little more than nucleus alone. It is only this part of the comet which can have any claim to solidity, or even appreciable weight. Though many of the telescopic comets are of extremely small mass, nucleus included—so small, indeed, that they are unable to perturb such small bodies as Jupiter's satellites—yet in some large comets the nucleus may be a few hundred miles in diameter, and may consist of solid matter. I need not say that the collision of a cometary nucleus of this order with the earth would be fraught with danger on a very wide scale.

2. *The coma.* This appears usually as a luminous fog surrounding the nucleus, and gradually shading off from it. The nucleus and the coma form together the head of the comet.

3. *The tail.* The tail may be considered as a continuation, in a direction opposite to that of the sun, of the luminous fog of the coma. This appendage may be scarcely distinguishable as a slight elongation of the coma, or it may extend half across the heavens, and be many millions of miles in length. The tail may be single, or composed of several branches.

We must now study more closely the cometary appearances as they may be seen when a large telescope is directed to a brilliant comet. I have selected for this purpose the Great Comet of 1858, and I shall exhibit on the screen a series of views of this comet, taken at intervals of a few days. The first set shows the growth, position, and forms of the tail, as a whole. The second group represents the more detailed structure and changes of form of the head of the comet, as viewed in a large telescope. These views are, of course, from sketches made at the telescope. Last year several attempts were made to photograph the comet which appeared in June. Mr. Janssen has kindly sent me a positive taken from the original negative. It is now upon the screen. Mr. Janssen purposely sacrificed detail in the head of the comet, for the sake of obtaining the structure and form of the tail, exposing the plate for thirty minutes. From a careful examination of several similar negatives, Janssen made a drawing of the comet. A photograph of this drawing is now upon the screen. Mr. Common, at Ealing, with a fine three-foot reflector of his own construction, also photographed the comet, but his object, different from that of Janssen, was to get the form of the nucleus. For this purpose he gave an exposure of only ten minutes—far too short to obtain an impression of the tail. The comet was also photographed by Dr. Draper, of New York. My own work was confined to the comet's spectrum, of which I shall speak presently.

We may now advance to the consideration of two primary questions:—

1. Does a comet shine wholly by reflected solar light, or has it also light of its own?

2. Of what materials is a comet composed?

The spectroscope has furnished us with information on both these points. The first successful application of the spectroscope to a comet was in 1864, when Donati discovered in its light three bright bands. In 1866 I was able to distinguish two kinds of light from a telescopic comet—the one kind giving a continuous spectrum and presumably solar light, and the other a spectrum of three bright bands, similar to those which had been seen by Donati. But in 1868 a great advance was made. The close agreement of measures I took of the bands of the comet *b* of that year with those I had previously taken of the spectrum of certain compounds of carbon led me to compare, directly, in conjunction with my friend Dr. W. Allen Miller, the spectrum of

the induction spark in olefiant gas with the comet's spectrum, in the manner shown upon the screen.

The next diagram shows the result of this direct comparison.* There could be no longer any doubt of the oneness of chemical nature of the cometary stuff with the gas we were using, in fact, that carbon, in some form or in some state of combination, existed in the cometary matter. From that time some twenty comets have been examined by different observers. The general close agreement, notwithstanding some small divergencies, of the positions of the three bands with those seen in the flame spectrum of hydrocarbons, leaves no doubt whatever that the original light of comets is really due to matter containing carbon in combination with hydrogen.

At first, indeed, for certain reasons, I was led to consider this spectrum to be that of carbon itself in the form of gas, a view still held by some physicists; but subsequent researches by several experimentalists on this point appear to me to be strongly in favour of carbon combined with hydrogen.†

Last year another advance was made. For the first time since the spectroscope has been in the hands of the astronomer the coming of a bright comet made it possible to extend this mode of research into the more refrangible region of the spectrum. Making use of the apparatus and arrangements which I employed for photographing the spectra of stars,‡ I succeeded in obtaining a photograph of the spectrum of the head of comet *b*.

A copy of this spectrum is now upon the screen (see plate). There is a continuous spectrum which can be traced from about G to beyond K, in which are seen distinctly several of the Fraunhofer lines, G, *h*, H, K, and many others. The presence of these lines was crucial, and made it certain that this continuous spectrum was really due to reflected solar light.§

But there was also present a second spectrum consisting chiefly of two groups of *bright* lines. These evidently were due to the same light which is resolved, in the visible region, into the three bright groups.

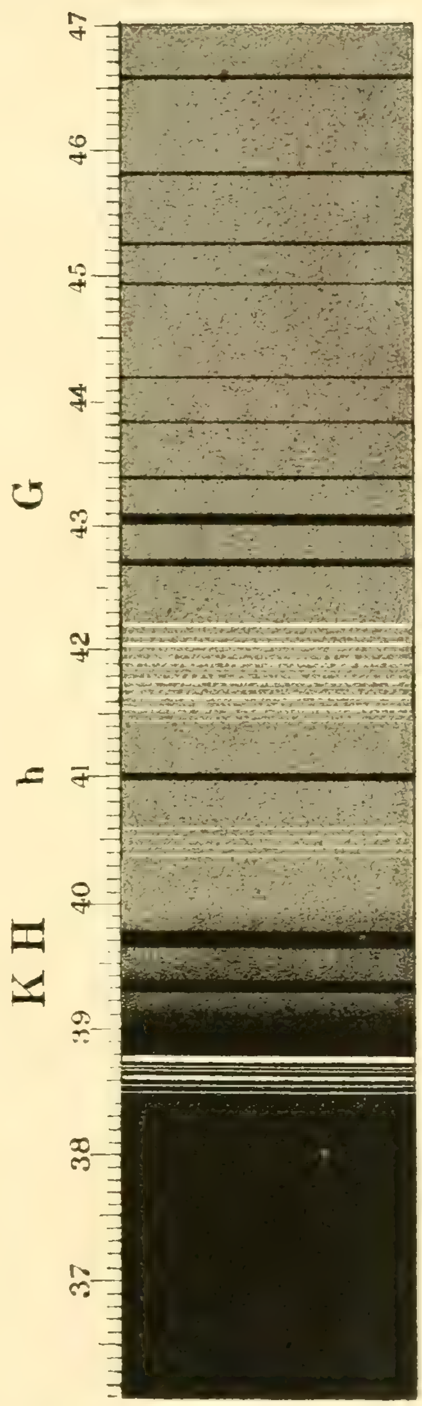
I regarded them with intense interest, for there was certainly hidden within these hieroglyphics some new information for us. Measures of their position in the spectrum, taken under the microscope, brought out that these groups were undoubtedly the same

* These hydrocarbon groups may be seen with a pocket spectroscope in the blue base of a candle flame or in the flame of a Bunsen burner.

† Among many papers, I may refer to 'Ueber die Spectra der Cometen,' Dr. Hasselberg, Mem. Acad. des Sciences, St. Pétersbourg, vii. ser. tome xxviii. No. 2; and papers by Professors Liveing and Dewar and by Mr. Lockyer in recent volumes of the 'Proceedings of the Royal Society.'

‡ 'Trans. R. S.' 1880, part 2, p. 671. 'Proceedings R. Instit.' vol. ix. part 3, p. 285.

§ See observations of the visible spectrum of this comet by Professor Young, the Astronomer Royal, Professor Vogel, Professor Wright, Dr. Von Konkoly, Dr. Hasselberg, and others.



Spottiswoode & Cº Lith London.

which appear in certain compounds of carbon. Professors Liveing and Dewar had recently shown that these groups indicate a *nitrogen* compound of carbon, namely, cyanogen. On this view there must be in the cometary matter, besides carbon and hydrogen, the element *nitrogen.*

A few days after my photograph was taken, Dr. Draper succeeded in obtaining a photograph of the comet's spectrum, which appears to confirm mine so far as the bright lines, but does not give the Fraunhofer lines.

About the same time that the observations were made on the comet, Professor Dewar succeeded in confirming his results, by the reversal of the groups, employing either titanic cyanide or boron nitride.

The positions and characters of these bands, together with those in the visible spectrum, leave no doubt that the substances, carbon, hydrogen, and nitrogen, and probably oxygen, are present in the cometary matter, and that this light-emitting stuff appears to be essentially of the same chemical nature for all the comets, some twenty, which have been observed up to the present time. Certain minor modifications of the common type of spectrum are often present, and show, as was to be expected, that the conditions prevailing in different comets, and indeed in any one comet from day to day, are not rigidly uniform.

The temperature, the state of tenuity, the more or less copious supply from the nucleus of the gaseous matter, must be subject to continual variation. At times it is probable that the hydrocarbon spectrum is complicated by traces of the spectrum of the oxygen compounds of carbon. These and other possible variations betray themselves to us in the spectrum, by the length of range of refrangibility through which each group can be traced, by an alteration in the position of maximum brightness in the groups, by the relative brightness of the groups, by a more or less breaking up of the shaded light of the bands and the visibility or otherwise of bright lines, by a more or less distinctness of the violet group, and, lastly, by the visibility in the brightest comet of last year of a less refrangible band of the hydrocarbon spectrum which occurs between C and D of the spectrum.*

We must now consider the information about the nature of comets which has come to us from a wholly different source.

On almost any fine night, after a short watch of the heavens, we shall see the well-known appearances of "shooting stars." At ordinary times, these are small, and appear indifferently in all parts of the heavens, but on certain nights they show themselves in great numbers, and of such brilliancy as to present a spectacle of much magnificence. On such occasions one remarkable feature presents

---

* For these reasons measures of these bands should be considered as strictly applicable to the particular comet at the time of observation only, and not necessarily as applicable to other comets.

itself, which is well marked in the diagram on the screen. The meteors all shoot forth from one spot, which is called the radiant point. A little consideration will show that this appearance is really due to perspective, and represents the vanishing point of the parallel courses in which the meteors are moving. Hence we learn that they all belong to an enormous swarm of these bodies which the earth is meeting, and further, we may find the direction in which the swarm is moving relatively to the earth. Now the researches of Olbers, H. A. Newton, and Adams showed that the November shower is really a planetary swarm, revolving round the sun in about $33\frac{1}{4}$ years. Further investigations of Schiaparelli, Leverrier, and Oppolzer brought out the astonishing result that the path of the November meteors is really identical with that of a comet discovered by Tempel in 1865. Schiaparelli showed further, that another independent group of meteors which appears in August, has an orbit identical with the third comet of 1862. We are thus led to see the close physical connection, and oneness of origin, if not indeed identity of nature, of comets and of these meteors. Now the meteors on these occasions are too minute to pass through the ordeal of ignition by our atmosphere, they are burnt up before they reach the earth, but at other times small celestial masses come down to us, which, there can be little doubt, are of the same order of bodies, and similar in chemical nature. The meteorites we have in our hands, contain matter of the same kind probably as that which gives rise to cometary phenomena. These two small meteorites, which fell at Estherville, were kindly sent to me by Professor Newton, as probably good examples of the sort of stuff of which the nuclei of comets are composed. The question arises, are the revelations of the spectroscope about comets in harmony with what we know of the chemical nature of these celestial waifs and strays?

Meteorites may be arranged in a long series, passing from metallic iron alloyed with nickel at one extremity, to those of a stony nature, chiefly silicates, at the other. In meteorites more than twenty of the elementary bodies have been found, including hydrogen, carbon, and nitrogen, which the spectroscope has shown to be in comets. It may be, however, that in the sun's action on comets, we have to do not with the decomposition of the cometary matter, but with the setting free of gases occluded within the meteoric matter, forming the comet's nucleus. If the meteoric matter were decomposed, we should expect a more complicated spectrum.

In the year 1867 Professor Odling, lecturing on Professor Graham's researches, lighted up this room with the gas brought by a meteorite from celestial space. This meteorite, of the iron type, yielded nearly three times its volume of gas, of which 85 per cent. was hydrogen, 5 per cent. was carbonic oxide, and 10 per cent. nitrogen. Since that time Professor A. W. Wright has experimented with a meteorite of the stony type, containing, however, numerous very small grains of metallic iron and sulphide of iron scattered

through the mass. This meteorite gave off about two and a half times the volume of the meteorite as a whole, or twenty times that of the iron scattered within it. The same gases came off, but in a different proportion; there being a larger proportion of the oxide of carbon, at a low temperature carbon dioxide was chiefly given off.* Now in all these cases, a spectrum similar to that of comets would be given by these gases under suitable conditions.

Some years ago, in conjunction with my friend Professor Maskelyne, I examined the spectra of certain meteorites, and obtained in several cases a spectrum similar to that of comets. Some meteorites like that of Bokkveldt, contain a large percentage of hydrocarbons. Professor Vogel has recently experimented in the same direction, and finds that the gas which comes off from the meteorite he used gives a hydrocarbon spectrum mixed with that of carbonic oxide, and under certain conditions the spectrum of hydrocarbon predominates and becomes almost exactly similar to that of comet *b* 1881.† We are at a disadvantage in one particular, for we cannot get at meteorites as they exist in celestial space, but only after superficial ignition in passing through the air.

The experiments hitherto made throw but little light on the question, whether cyanogen ready formed is present in combination or otherwise in the comet, or whether it is formed at the time by the interaction of carbonaceous and nitrogenous matter. In the latter case we should have to admit a high temperature, which would be in favour of the view of an electric origin of the comet's light. Professor A. Herschel and Dr. Von Konkoly have pointed out that the spectra of the periodic meteors are different for different groups. I may also mention that Captain Abney considers that he has evidence of hydrocarbons in the outer portion of the sun's atmosphere.

---

* 'American Journal of Science and Arts,' vol. x. July 1875.

† 'Publicationen des Astrophysikalischen Observatoriums,' Band ii. p. 182.

Since this Discourse was given, Dr. Flight has presented to the Royal Society a paper on the Meteorite of Cranbourne, Australia, and the Rowton Meteoric Iron. In the case of the former, the occluded gases amounted to 3·59 the volume of the iron, and consisted of—

Carbonic acid	0·12
Carbonic oxide	31·88
Hydrogen	45·79
Marsh gas	4·55
Nitrogen	17·66
	100·00

The Rowton Iron gave 6·38 times its bulk of gas, as follows—

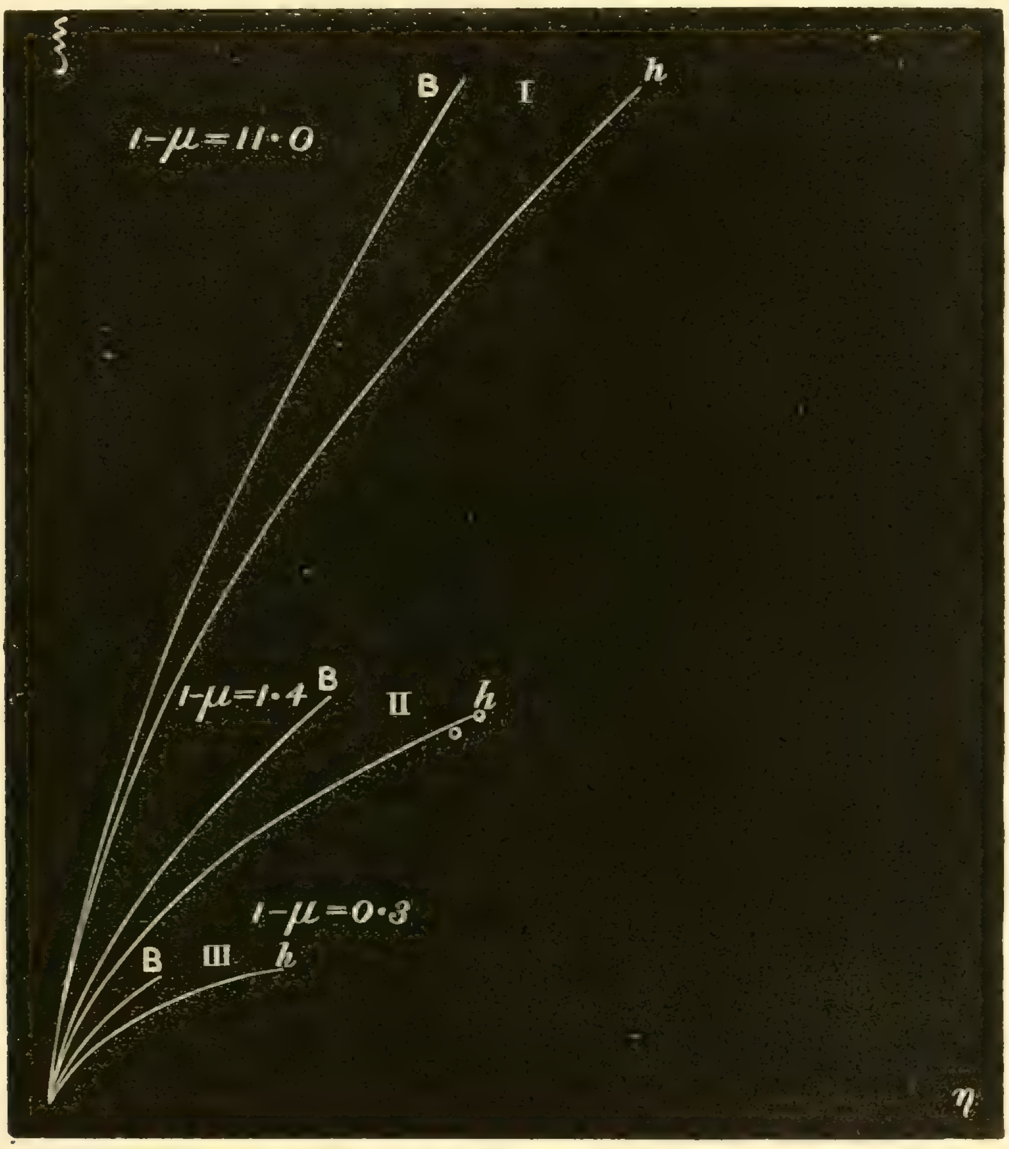
Carbonic acid	5·155
Hydrogen	77·778
Carbonic oxide	7·345
Nitrogen	9·722
	100·000

We have now advanced to the extreme boundary of the solid ground of our knowledge of comets. Before us lies the enchanted region of speculation. Without being too venturesome, we may well consider a few points which may explain more in detail some of the phenomena of comets. Of whatever nature we may regard the tremendous changes which take place in them to be, we must certainly look for the primary disturbing cause to the sun. Is the solar heat sufficient to account directly for the self-light of comets, or does it act the part of a trigger, setting free chemical or electrical forces? On this point of the sufficiency of the solar radiation we must not look to the few cases of exceptionally close approach to the sun, but to the more average distance of comets at perihelion. Professor Stokes has suggested that some results obtained by Mr. Crookes may throw light upon this question. He concluded from his experiments that in such vacua as exist in planetary space the loss of heat, which in such cases would take place only by radiation, would be exceedingly small.* In this way the heat received from the sun by the comet would accumulate, and we should get a much higher temperature than would otherwise be possible. In this connection may be mentioned the remarkable persistence of the bright trains of meteors in the cold upper air, which sometimes remain visible for three-quarters of an hour before the light fades out by the gradual dissipation of the energy.

I need hardly say that the enormous tails of bright comets, many millions of miles in length, cannot be considered as one and the same material object, brandished round like a great flaming sword, as the comet moves about the sun. It is but little less difficult to suppose that the cometary mass is of so large an extent as to include all the space successively occupied by the sweep of the tail at perihelion. On the material theory we seem to be shut up to the view that the tail is constantly renewed and reformed, either by matter streaming from the nucleus or in some other way. But this view involves velocities far greater than the force of gravitation can account for. Let us consider the order of the phenomena. Under the sun's influence, luminous jets issue from the matter of the nucleus on the side exposed to the sun's heat. These are almost immediately arrested in their motion sunwards, and form a luminous cap; the matter of this cap then appears to stream out into the tail, as if by a violent wind setting against it. Now, one hypothesis supposes these appearances to correspond to the real state of things in the comet, and that there exists a repulsive force of some kind acting between the sun and the gaseous matter, after it has been emitted by the nucleus. On this hypothesis the forms of the tails of comets, which are usually curved, and denser on the convex side, admit of explanation. Each particle of matter of the tail must be moving in a curved course, under the influence of the motion it originally possessed, combined with that of this hypothetical repulsive force. But in the form which the tail assumes for us we

* 'Proceedings R. S.' 1880, p. 243.

have not only to consider the effect of perspective, but also that the comet itself is advancing, so that the visible tail is due to the portion of space which at the time contains all the repelled matter, each particle describing its own independent orbit, and reflecting to the eye the solar light or giving out its own light, as the case may be.* The value of the repulsive force which would be necessary on this



theory has been investigated by Bessel, Peirce, and others.† Recently Bredichin ‡ has investigated the curvatures of the tails of a number of comets. According to him, they fall into three classes, which are represented in this diagram, each type of curve depending upon a different assumed value of the repulsive force. This leads to another point, namely, the secondary tails which are often present. Some of these appear to be darted off with an energy of repulsion so enor-

---

* As a rule, the tails of comets appear to be luminous by reflected solar light, but at times the stuff which emits the light giving a spectrum of bright bands is carried into the tail to a greater or less distance from the head.

† See numerous papers by Faye in the 'Comptes Rendus.'

‡ 'Annales de l'Observatoire de Moscou,' vol. v. liv. 2, p. 30; and 'Astr. Nachr.' No. 2411.

mously great that the original motion of the nucleus tells for very little, and hence the secondary tail is but slightly curved, or even is sensibly straight. Again, if we take the hypothesis that this repulsive force, of whatever character it may be, varies as the surface, and not, like gravity, as the mass, substances of different specific gravity would be differently affected and separated from each other, and these secondary straight, or nearly straight tails would, on this view, consist of the lightest matter.

On this hypothesis a comet would suffer of course a large waste of material at each return to perihelion, as the nucleus would be unable to gather up again to itself the scattered matter of the tail; and this view is in accordance with the fact that no comet of short period has a tail of any considerable magnitude.

A theory, based on chemical decomposition, has been proposed by Professor Tyndall,* but as this view has been illustrated here by the eloquent author himself, I will not now enter upon it.

A different view of the whole matter has been suggested by Professor Tait.† He supposes, not the nucleus only, but the whole comet, to consist of a swarm, of enormous dimensions, of minute meteoroids, which become self-luminous at and about the nucleus, in consequence of the impacts of the various meteoric masses against each other, giving rise to incandescence, melting, the development of glowing gas, and the crushing and breaking up of the bodies into fragments of different sizes, and endowed with a great variety of velocities. The tail he conceives to be a portion of the less dense part of the train illuminated by sunlight, and visible or invisible to us, according not only to circumstances of density, illumination, and nearness, but also of tactic arrangement, as of a flock of birds under different conditions of perspective, or the edge of a cloud of tobacco smoke.

On this hypothesis we should expect to find a more complicated spectrum, and the spectra of comets to differ greatly from each other.

There seems to be a rapidly-growing feeling among physicists that both the self-light of comets and the phenomena of their tails belong to the order of electrical phenomena. One of the most distinguished of the American astronomers wrote to me recently: "As to the American views of the self-light of comets I cannot speak with authority for any one but myself, still I think the prevailing impression amongst us is that the light is due to an electric, or, if I may coin the word electric-oid action of some kind." Here I confess I tread most cautiously, for we have no longer any stepping-stones of fact on which to place our feet. I am ready to admit that the spectroscopic evidence, especially that furnished by the photographs of last year, favours, though it does not necessarily demand, the view that the self-light of comets is due to electric discharges. I do not attach

* Phil. Soc. Cambridge, and 'Phil. Mag.' April 1869.

† 'Proceedings R. Society Edinburgh,' vol. vi. p. 553.

much importance to the fact that the bright groups in the visible spectrum of comet *b* agreed with those of the so-called "flame spectrum," for the reason that the same spectrum may be obtained from the induction spark, when suitable arrangements are used to make the discharge one of comparatively low temperature.*

As we are now fairly on the wide ocean of speculation, I need not say that the precise modes of application of the principle of electricity which have been suggested are many. Broadly, they group themselves about the common idea that great electrical disturbances are set up by the sun's action in connection with the vaporization of some of the matter of the nucleus, and that the tail is matter carried away, possibly in connection with electric discharges, in consequence of the repulsive influence of the sun, which is supposed to be in a state of constant high electrical potential of the same name. Further, it is supposed that the luminous jets and streams and caps and envelopes belong to the same order of phenomena as the aurora, the electrical brush, and the stratified discharges of exhausted tubes. Views resting more or less on this basis have been suggested by several physicists, and, in particular, have been elaborated at great length by Zöllner, who endeavours to show that on certain assumed data, which appear to him to be highly probable, the known laws of electricity are fully adequate to the explanation of the phenomena of comets.†

All the theories we have considered assume that the bright lines seen in the spectra of comets indicate heated luminous gas. An alternative hypothesis has been suggested by Professor Wright,‡ and especially by Mr. Johnstone Stoney,§ who considers that the compound of carbon vapour is opaque in reference to the particular rays which appear as bright lines, and they appear as bright lines in consequence of sending back to us the sun's rays falling upon the vapour. Further, he considers the phenomenon to be of the order of phosphorescent bodies, and he states that the conditions existing in the cometary gas are such as will eminently promote phosphorescence, and therefore visibility, in presence of a luminary.||

Here I must stop. May I venture to hope that the experience of the past hour has not been such as to confirm in your minds the old view to which I referred at the beginning of the lecture, that the influence of comets is always a malign and woeful one. [W. H.]

---

* See Professor Piazzi Smyth, 'Nature,' vol. xxiv. p. 430.

† 'Astr. Nachr.' Nos. 2057–2060, 2082–2086, and 'Ueber die Natur der Cometen,' Leipzig, 1872.

‡ 'American Journ. S. and A.' vol. x. July 1875.

§ British Association Report, 1879, p. 251.

|| Respighi ('Comptes Rendus,' 5 Sept. 1882) has sought indeed to explain the occurrence of bright bands by supposing them to be simply the remaining portions of the continuous spectrum of reflected sunlight after absorption through the enormous depth of the comet's atmosphere. This view appears to me for many reasons improbable, especially if we take into account the extreme relative brilliancy of the most refrangible group in the photographic spectrum of comets.

WEEKLY EVENING MEETING,

Friday, January 27, 1882.

William Bowman, Esq. LL.D. F.R.S. Honorary Secretary and Vice-President, in the Chair.

Reginald Stuart Poole, Esq. of the British Museum, Cor. Inst. France.

The Museum and Libraries of Alexandria.

The speaker stated that his object was to show the connection between the ancient Egyptian and Alexandrian educational institutions, and expressed his gratitude for the invaluable aid of the eminent French Egyptologist, M. Revillout.

The sources of information are chiefly old hieratic papyri, some of which are actually exercise-books of students, and they tell us of colleges attached to temples in various towns. When Plato and others visited Egypt, Heliopolis was most famous. The subjects taught were religion, law, mathematics, especially geometry and astronomy, medicine and language. There were also primary schools for all classes. Libraries were attached to the temples, and there was a royal library existing at least as early as B.C. 2500.

The Alexandrian foundations were due to the wisdom with which the first three Ptolemies carried out the large-minded policy of Alexander the Great. They were meant to benefit the mixed population of Alexandria—Egyptian, Greek, and Hebrew.

The Museum was a sacred building in the palace, where learned men were maintained by the State to prosecute research. Law and religion were excluded in order to avoid controversy. A botanical garden and a menagerie were added.

Besides the similarity of scheme, and the evident succession of Alexandria to Heliopolis, a strong point of contact was the old method, as seen in the mathematical processes of the second Heron.

To the first library, originally Greek only, translations were added, and the temple of Sarapis received surplus books. The first library was burnt when Julius Cæsar captured Alexandria. The second, enriched by Antony with the Pergamus collection, is said to have been burnt at the Arab conquest, when it disappeared.

The effect of the Alexandrian foundations was very great. The intelligence of the East and West here met, and it is due to this that the Old Testament was translated into Greek.

The Alexandrian University was restored by an Arab Prince, the caliph El-Mutawekkil, two centuries after the conquest; and the

great University of Cairo was founded by a Greek officer of the Fatimite caliph in A.D. 969–70.

The University of Cairo practically includes all the Alexandrian faculties except medicine, which is considered by the Arabs to be unsuited to public education. Lately, of 5000 students 2500 were there educated and maintained free of all cost to themselves. The professors, who now receive moderate rations from the State, make a modest income by outside teaching and copying MSS.

WEEKLY EVENING MEETING,

Friday, February 3, 1882.

William Spottiswoode, Esq. M.A. D.C.L. Pres. R.S.
Vice-President in the Chair.

Professor Tyndall, D.C.L. F.R.S. *M.R.I.*

Action of Molecules, Free and Constrained, on Radiant Heat.

(Abstract deferred.)

GENERAL MONTHLY MEETING,

Monday, February 6, 1882.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Sidney Biddell, Esq. M.A.
The Earl of Dysart,
Mrs. Archibald Hamilton,

were elected Members of the Royal Institution.

Report from the Managers.

The following Resolution passed by the Committee of Managers at a Special Meeting held on December 16, 1881, was read and adopted by the Members:—

1. The Board of Managers of the Royal Institution received with great regret Mr. Warren De La Rue's letter of December 3, 1881, addressed to Professor Tyndall, announcing that the state of his health compelled him to resign the office of Honorary Secretary to the Royal Institution.
2. The Managers fully appreciated the considerate offer made by Mr. De La Rue to continue in the post of Honorary Secretary for some time longer, if such a course were deemed desirable for the advantage of the Institution; but they believed it to be their duty to secure for Mr. De La Rue the immediate release from the cares of office which seemed indispensable.
3. The Managers trust that Mr. De La Rue may be enabled, after due rest and medical treatment, to resume those scientific pursuits in which so much of his life has been spent, and to the prosecution of which by others so much generous assistance has always been extended by him.
4. The Managers cannot bid farewell to Mr. De La Rue, as Honorary Secretary to the Royal Institution, without tendering to him their warmest thanks for the ability, zeal, and liberality with which he has aided the Royal Institution while filling that important office.
5. The devotion of Mr. De La Rue's time and attention to the interests of the Institution will always be remembered with gratitude in connection with the services of other distinguished men, many of whose names, like his own, belong to the history of science—a history to which the work done in the Royal Institution has made such signal contributions.

William Bowman, Esq. LL.D. F.R.S. was elected Honorary Secretary, and Warren De La Rue, Esq. M.A. D.C.L. F.R.S. was elected Manager.

Thirteen Candidates for Membership were proposed for election.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor General of India—Geological Survey of India:
Records. Vol. XIV. Part 4. 8vo. 1881.
Palæontologia Indica: Series XIV. Vol. I. Part 3, Fasc. 1.

The Lords Commissioners of the Admiralty—Account of British Observations of the Transit of Venus, Dec. 8, 1874. Edited by Sir G. B. Airy. 4to. 1881.
New Zealand Government—Statistics for 1880. fol. 1881.
Accademia dei Lincei, Reale, Roma—Atti, Serie Terza: Vol. VI. Fasc. 2, 3, 4. 4to. 1881.
Actuaries, Institute of—Journal. No. 124. 8vo. 1881.
Asiatic Society of Bengal—Journal. Vol. L. Part I. Nos. 3 and 4. Part II. No. 4. 8vo. 1881.
Proceedings, No. 9. 8vo. 1881.
Asiatic Society, Royal—Journal, New Series, Vol. XIV. Part. 1. 8vo. 1882.
Astronomical Society, Royal—Monthly Notices, Vol. XLII. Nos. 1, 2. 8vo. 1881.
Ball, Professor R. S. LL.D. F.R.S. (the Author)—A Glimpse through the Corridors of Time. 8vo. 1882.
Bankers' Institute—Journal, Vol. II. Part 10. Vol. III. Part 1. 8vo. 1881–2.
Bartholomew's Hospital—Statistical Tables for 1880. 8vo. 1881.
Batavia Observatory—Rainfall in the East Indian Archipelago, 1880. By P. A. Bergsma, the Director. 8vo. Batavia, 1880.
Magnetical and Meteorological Observations, Vol. V. 1879–80. 4to. 1881.
Bayley, Francis, Esq. M.R.I. (the Author)—The Bailleuls of Flanders. 8vo. 1881. (Privately Printed.)
Bramwell, Sir Frederick, F.R.S. M.R.I. (the Author)—Address to the Society of Arts. 8vo. 1881.
British Architects, Royal Institute of—Proceedings, 1881–82, Nos. 5, 6, 7, 8. 4to. 1881–2.
British Museum Trustees:
Cuneiform Inscriptions of Western Asia. Vol. V. Assyria. fol. 1881.
Photograph of Shakspere Deed. 1881.
Catalogue of Oriental Coins. Vols. IV. V. and VI. 8vo. 1879–81.
Lepidoptera Heterocera. Parts III. IV. and V. 4to. 1879–81.
Catalogue of Birds. Vol. V. 8vo. 1881.
Catalogue of German and Flemish Prints. Vol. I. 8vo. 1879.
New Species of Hymenoptera. 8vo. 1879.
Types of Coleoptera. Part I. 8vo. 1879.
Catalogue of Persian MSS. Vol. II. 4to. 1881.
Hand List of Bibliographies, &c. 8vo. 1881.
Index of Minerals. 8vo. 1881.
Chemical Society—Journal for Dec. 1881 and Jan. 1882. 8vo.
Civil Engineers' Institution—Proceedings, 1881–2. Nos. 2–6. 8vo.
Conférence Polaire Internationale, St. Pétersbourg—3e Rapport sur les Actes et Résultats. 4to. 1881.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Series II. Vol. I. Part 6. 8vo. 1881.
Crookes, W. Odling, W. and C. Meymott Tidy (the Authors)—Reports on London Water Supply, 1880–1. Nos. 10, 11, 12. 4to.
Dax: Société de Borda—Bulletins, 2e Série sixième Anné: Trimestre 4. 8vo. Dax, 1881.
East India Association—Journal, Vol. XIII. No. 3. 8vo. 1881.
Editors—American Journal of Science for Dec. 1881 and Jan. 1882. 8vo.
Analyst for Dec. 1881 and Jan. 1882. 8vo.
Athenæum for Dec. 1881 and Jan. 1882. 4to.
Chemical News for Dec. 1881 and Jan. 1882. 4to.
Engineer for Dec. 1881 and Jan. 1882. fol.
Horological Journal for Dec. 1881 and Jan. 1882. 8vo.
Iron for Dec. 1881 and Jan. 1882. 4to.
Nature for Dec. 1881 and Jan. 1882. 4to.
Revue Scientifique and Revue Politique et Littéraire, Dec. 1881 and Jan. 1882. 4to.
Telegraphic Journal for Dec. 1881 and Jan. 1882. 8vo.
Eklund, A. F. Esq. (the Author)—Contribution a la Géographie Médicale. La Nouvelle Caserne des Recrues de Skeppsholm au Point de Vue Hygiénique. 8vo. Stockholm, 1881.

Franklin Institute—Journal, Nos. 672, 673. 8vo. 1881–2.
Geographical Society, Royal—Proceedings, New Series. Vol. III. No. 12. Vol. IV. Nos. 1, 2. 1881–2.
Geological Society—Quarterly Journal, No. 148. 8vo. 1881.
Abstracts of Proceedings, 1881–2, Nos. 408–413. 8vo.
Geological Society of Ireland, Royal—Journal, New Series. Vol. VI. Part 1. 8vo. 1881.
Greenwich Observatory—Spectroscopic and Photographic Results. 4to. 1880.
Harlem Société Hollandaise des Sciences—Archives Néerlandaises, Tome XVI. Liv. 3, 4, 5. 8vo. 1881.
Natuurkundige Verhandelingen. 3de Verz. Deel IV. 2de Stuk. 4to. 1881.
Helps, Williams, Esq. M.R.I.—The Astral Origin of the Emblems, the Zodiacal Signs and Hebrew Alphabet. By the Rev. J. H. Broome. 4to. 1881.
Hudleston, W. H. Esq. M.A. F.G.S. M.R.I. (the Author)—Geology of the Vale of Wardour. (Proceedings of the Geologists' Association, Vol. VII.) 8vo. 1881.
Linnean Society—Journal, Nos. 89, 90, 115, 116. 8vo. 1881–2.
Lisbon, Académie Royale des Sciences—Memorias: Sciencias Mathematicas, &c. Tomo VI. Parte 1. 4to. 1881.
Liverpool Polytechnic Society—Journal and Annual Report, 1881. 8vo. 1881.
London Library—Catalogue, Supplemental Volume, 1875–80. By R. Harrison. 8vo. 1881.
McCosh, John, M.D. &c. (the Author)—A Proposal for a Floating Harbour of Refuge. (O 17) 12mo. 1881.
Mechanical Engineers, Institution—Proceedings, 1881. No. 4. 8vo. 1881.
Meteorological Office—Quarterly Weather Reports, Part 1, Jan.–Mar. 1876. 4to. 1881.
Meteorological Society—Meteorological Record, No. 3. 8vo. 1881.
Index to Publications of the English Meteorological Societies (1839–81). 8vo. 1881.
Montpellier Académie des Sciences et des Lettres—Mémoires, Tome X. Fasc. 1. 4to. 1881.
National Association for Social Science—Sessional Proceedings. Vol. XV. Nos. 2, 3. 8vo. 1881.
North of England Institute of Mining and Mechanical Engineers—Transactions Vol. XXX. 8vo. 1881.
Pharmaceutical Society of Great Britain—Journal, Dec. 1881 and Jan. 1882. 8vo.
Photographic Society—Journal New Series, Vol. VI. Nos. 3, 4. 8vo. 1881.
Plateau, M. J. Hon.M.R.I. (the Author)—Une Application des Images Accidentales (10e Note). (Bulletins de l'Académie Royale de Belgique, 3e Série, Tome II. Nos 9–10.) 8vo. 1881.
Preussische Akademie der Wissenschaften—Monatsberichte: Nov. 1881. 8vo.
Ramsay, A. Esq.—Scientific Roll, Part I. No. 5. 8vo. 1881.
Royal Society of London—Proceedings, No. 216. 8vo. 1881.
Société Zoologique de France—De la Nomenclature des Etres Organisés. 8vo. Paris, 1881.
Society of Arts—Journal, Dec. 1881 and Jan. 1882. 8vo.
Spedding, Miss—Evenings with a Reviewer; or, Macaulay and Bacon. By J. Spedding. With a Prefatory Notice by G. S. Venables. 2 vols. 8vo. 1881.
Stanley, Wm. Ford, Esq. M.R.I. (the Author)—Experimental Researches into the Properties and Motions of Fluids. With Theoretical Deductions therefrom. 8vo. 1881.
Statham, H. H. Esq. (the Author)—Notes on Ornament. (Lectures delivered at the Royal Institution.) (Portfolio, Jan. 1882.)
Statistical Society—Journal, Vol. XLIV. Part 4. 8vo. 1881.
St. Pétersbourg, Académie des Sciences—Memoires, 7e Série, Tome XXVIII. Nos. 8, 9. Tome XXIX. No. 1. 4to. 1881.
Symons, G. J.—Monthly Meteorological Magazine, Dec. 1881 and Jan. 1882. 8vo.
United Service Institution, Royal—Journal, No. 113. 8vo. 1881.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1881, No. 10. 4to.

WEEKLY EVENING MEETING,

Friday, February 10, 1882.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

E. Frankland, Esq. D.C.L. F.R.S. *M.R.I.*
Professor of Chemistry in the Normal School of Science, South Kensington Museum.

Climate in Town and Country.

The speaker began by describing the construction and uses of the instruments with which he had studied the conditions of climate, for many years past, in various parts of Europe. For the determination of sun temperature, he used a thermometer technically known as the blackened bulb *in vacuo* laid in full sunshine upon a sheet of white paper. The shade or air temperature was measured by an ordinary thermometer with a clear glass bulb and a scale engraved upon the stem. This thermometer was placed upon the same sheet of paper, and was shaded by a small white paper arch which admitted of a free circulation of air around the bulb.

He then explained the terms " sun temperature," " shade temperature," and " solar intensity." By shade temperature is meant the temperature of free air in full sunshine. Strictly it ought to be ascertained without any shade at all; for as soon as a shade is produced, conditions are introduced which often entirely baffle the object of the observer. The shade of a parasol has a different temperature from the shade of a tree, and this, again, differs widely from that of a house. The temperature of the shade of a sheet of tinfoil is quite different from that of a sheet of writing paper. Indeed it may be truly said that every shade has its own peculiar temperature. The following table shows the effect of the area of shade, and of the quality of the shading material:—

	°
Beneath larch tree	19·5 C.
„ white parasol	25·0
„ small white paper arch	35·0
„ small arch of bright tinfoil	45·2

Thus shade temperatures, measured during $1\frac{3}{4}$ hours of uninterrupted sunshine in the middle of the day, and within a few yards of the same spot, differed by no less than 25·7° C. These observations were, however, made at Pontresina, 5915 feet above sea-level, and so wide a range does not occur at lower altitudes.

The most effective shading material is, obviously, that which most perfectly reflects solar heat; and of all materials with which he had experimented white paper was found to be the best, white linen and

zinc-white being nearly equal to it. The most trustworthy shade thermometer, therefore, is one having its bulb covered with a thin layer of one of these materials; or the naked bulb may be shaded by a small arch of white paper.

The term "sun temperature," as commonly employed, has a very vague meaning. If a body could be placed in sunlight under such circumstances as to absorb heat rays and emit none, its temperature would soon rise to that of the sun itself. But, as all good absorbers of heat are also good radiators, the elevation of temperature caused by the exposure of even good absorbers to sunlight is comparatively small. Thus an isolated thermometer, with blackened glass bulb, placed in sunshine, will rarely rise more than 10° C. above the temperature which it marks when screened from direct sunlight. Under these circumstances, however, the thermometer loses heat not merely by radiation, but also by actual contact with the surrounding cold air. If the latter source of loss be obviated, a much higher sun temperature is obtained. Thus, the blackened bulb enclosed in a vacuous clear glass globe will sometimes, when placed in sunlight, rise as much as 60° C. above the shade temperature, and a still higher degree of heat may be obtained by exposing to the sun's rays the naked blackened bulb of a thermometer enclosed in a wooden box padded with black cloth, and closed by a lid of clear plate glass. Thus he obtained with such a box, on the 22nd of December, in Switzerland, when the air was considerably below the freezing point, a temperature of 105° C., and a still higher temperature could doubtless be obtained by surrounding the thermometer with a vacuous globe before enclosing it in the padded box. These widely different temperatures, produced under different conditions by the solar rays, show that such observations can be comparative only when the thermometer employed to measure them is always surrounded by the same conditions. All the sun temperatures here mentioned were measured when the "blackened bulb *in vacuo*" was laid horizontally upon a sheet of white paper with its stem at right angles to the direction of the sun's rays.

"Solar intensity" is relative only, and means the number of degrees through which the sun raises the temperature of a blackened bulb *in vacuo* over the shade temperature. Hence the two temperatures must be observed simultaneously, which is a laborious operation when continued half-hourly throughout the day. By the use of a peculiar self-registering differential thermometer, however, which he had recently described to the Royal Society,* the maximum solar intensity during the day is recorded by one reading only. The solar intensities commented upon in this discourse were ascertained by subtracting, in each case, the shade temperature from the sun temperature taken synchronously. The precautions necessary are described in the paper to the Royal Society just quoted.

The chief things affecting climate are the following:—(1) The

---

* 'Proceedings of the Royal Society,' 1882, p. 331.

sun. (2) Land and water—ocean and atmospheric currents. (3) Aspect—slope of ground, exposure or shelter. (4) Nature of surface. (5) Reflection from land and water. (6) Rain and clouds—suspended matter in the air. (7) Latitude—incidence of solar rays, thickness of air. (8) Presence or absence of aqueous vapour. Of these, the first three are obvious and require no comment. The remainder are less well known, but their importance demands our special attention.

Climate, or rather genial climate, is ultimately resolvable into two prime factors—sun-warmth and air-warmth. The amount of sun-warmth (assuming the sun's heat to be constant) depends upon two things only—length of day, and quantity of suspended matter and aqueous vapour in the air. The warmth of the air depends upon contact with matter heated by the sun's rays and upon the stoppage of radiation from the earth by aqueous vapour. This heated matter is:—(1) Sea or land. (2) Suspended matter in the air—cloud, dust, smoke. (3) Aqueous vapour.

These two factors were first considered in their relation to

Country Climate.

The feeling of warmth and comfort in the open air is produced either by direct solar radiation, even if the air be very cold; or by the warmth of the air itself. Upon both of these, the nature of the surface upon which the sunlight falls has a paramount influence, as is seen from the results of experiments on sun temperature recorded in the following table:—

Influence of Surface.

Norway.	
Green grass	57°·3 C.
Parched grass	61·2
Bare soil	60·6
Newly-mown grass	56·5
White paper	73·5
Hesse Cassel.	
Black caoutchouc	54°·7 C.
Black silk	56·5
Plane glass mirror	64·0
Slightly concave metallic mirror	64·0
Green grass	58·5
White paper	67·7
Switzerland. Mortaratsch Glacier.	
Black caoutchouc	39°·0 C.
Bare white ice	47·5
White paper	53·0
Summit of Gornergrat.	
Dazzling white snow	59°·0 C.
White paper	61·2
Pontresina.	
White paper	66°·2 C.
Grass	54·0
Grey rock	54·0
Black caoutchouc	56·4

Diavolezza.

Black caoutchouc	39°·1 C.
Snow	61·9
White paper	65·8

Italy. Bellagio.

Black caoutchouc	60°·0 C.
Black merino	59·0
White linen	66·0
White paper	66·3

These results may be imitated with the powerful light from a Siemens' dynamo-machine. [Experiments shown.]

The warmth of the air over these surfaces was in the inverse order, caoutchouc heating the air most, white paper and snow least. The nearer the colour of the ground approaches to *white*, the more genial will be the climate from radiation and the cooler will be the air. The nearer it gets to *black*, the warmer will be the air and the less will temperature be due to radiation. Dark surfaces warm the air; light surfaces keep it cool, but warm the body by radiant reflection. The difference is substantially the same out of doors as that produced indoors by a close stove on the one hand, and an open fire on the other; but calm air is required for the enjoyment of radiant heat.

The sun's radiant heat may be greatly reinforced by reflection from surrounding objects. There are two kinds of reflectors; those which, like white paper, white linen, and whitewash, scatter the solar heat in all directions, and those which, mirror-like, reflect it in one direction only. To the former belong snow, chalk, light-coloured sand, and light-coloured earth; to the latter, water. The former are useful on whatever side they may be, the latter only when they are between the observer and the sun. The observations in the following table illustrate this effect of reflection from surrounding objects:—

Influence of Reflection from Surrounding Objects.

From a white-washed wall. Pontresina.

On white paper 10 feet from wall	38°·7 C.
„ in adjoining meadow	27·7

From water. Top of cliff at Alum Bay, Isle of Wight.

Direct and reflected rays	31°·2 C.
Direct rays only	25·7

Zürich. One mile from Lake.

Direct and reflected rays	34°·0 C.
Direct rays only	31·5

M. Dufour has observed the same phenomenon on the lake of Geneva between Lausanne and Vevay. He has measured the proportions of direct and reflected heat at five different stations on the northern shore of the lake, and the results are condensed in the following table:—

DUFOUR'S OBSERVATIONS.

Altitude of Sun.	Proportion of direct to reflected heat.
3° 34′ to 4° 38′	100 : 68.
7°	100 : 40 to 50.
16°	100 : 20 to 30.

When the sun was higher than 30° the reflected heat was hardly perceptible. Hence this reflection is of the greatest value in winter, when it is most wanted, and it also tends to equalise temperature during the day; for in the early morning and evening, when the sun is low, and his direct heat is small, the reflected heat is greatest.

The bearing of these observations upon winter refuges for invalids is obvious. While the primary conditions to be secured must ever be fine weather and a sheltered position, the next in importance is, doubtless, exposure all day long to reflected, as well as direct, solar radiation. To realise this, a southern aspect and a considerable expanse of water or snow are necessary, and it is important that the sanitarium should be considerably and somewhat abruptly elevated above the reflecting surface, so that it may receive, throughout the entire day, the uninterrupted reflection of the sun's rays. At or near the sea-level, however, it is impossible, owing to solid and liquid matters floating in the lower regions of the atmosphere, to enjoy anything approaching to a uniform temperature from sunrise to sunset.

Although this suspended matter exists even at great altitudes, the bulk of it floats below 5000 feet, and whilst only one-sixth of the atmosphere is below this height, there is probably much more than one-half of the suspended matter at a lower elevation. As might be expected, therefore, solar intensity is much greater at high than at low elevations, although the temperature of the air continually decreases as it is further removed from the earth's surface. The following tables contain observations illustrative of this point:—

SOLAR INTENSITY.

Station.	Height of Barometer.	Sun's Altitude.	Indicated Solar Intensity.
	inch.	°	° C.
Oatlands Park	29·9	60	41·5
Riffelberg	22·0	60	45·5
Hörnli	21·2	61	48·1
Gornergrat	20·5	61	47·0
Isle of Wight	30·0	58	42·3
Riffelberg	22·0	60	45·5
Piz Languard	20·2	54	45·8
Whitby	30·1	50	37·8
Pontresina	24·0	49	44·0
Bernina Hospitz ..	22·6	51	46·4
Diavolezza	20·8	50	59·5
Bellagio	29·3	47	39·8
Shiahorn	21·6	46	43·5
Schwarzhorn	20·3	46	45·5

Shade Temperatures at Noon and different Altitudes.

Station.	Height above Sea.	Sun's Altitude.	Temperature.
	feet.	°	° C.
Oatlands Park	150	60	30·0
Riffelberg	8,428	60	24·5
Hörnli..	9,491	61	20·1
Gornergrat..	10,289	61	14·2
Whitby	60	50	32·2
Aak, Romsdal	20	49	36·2
Pontresina	5,915	49	26·5
Bernina Hospitz ..	7,644	51	19·1
Diavolezza	9,767	50	6·0
Bellagio	700	47	28·5
Shiahorn	8,924	46	23·0
Schwarzhorn	10,338	46	20·5

Hence it follows that the difference of solar intensity between noon and sunrise and sunset respectively is less at great than at small elevations, a deduction which is substantiated by the experimental data contained in the following table :—

Variation of Solar Intensity at Different Hours.

Station.	Time.	Solar Intensity.	Difference.
		° C.	° C.
Isle of Wight	Noon	42·3	7·6
,, ,, ...	3.30 P.M.	34·7	
,, ,, ...	Noon	42·1	8·5
,, ,, ...	3.15 P.M.	33·6	
,, ,, ...	Noon	41·7	8·4
,, ,, ...	3.50 P.M.	33·3	
At Sea	8.30 A.M.	33·8	7·9
,, ...	Noon	41·7	
Riffelberg (8428 ft.) ..	8.20 A.M.	40·9	4·6
,, ,,	Noon	45·5	
Gornergrat (10,289 ft.)	,,	47·0	5·3
,, ,,	3 P.M.	41·7	

Similar testimony is also afforded by a comparison of early and late observations at widely different altitudes :—

Variation of Solar Intensity at Different Altitudes.

Station.	Time.	Sun's Altitude at Noon.	Height above Sea.	Solar Intensity.	Difference.
	A.M.	°	feet.	° C.	° C
At Sea.. ..	7.35	72	0	28·6	8·6
Riffelberg ..	7.45	60	8428	37·2	
At Sea	8.8	72	0	30·3	10·6
Riffelberg ..	8.20	60	8428	40·9	

The sun's altitude was unfavourable for the comparison; nevertheless, there were here observed differences of 8·6 C. and 10·6°.

The farther we recede from the earth, the nearer we realise the conditions of solar radiation altogether outside the limits of the atmosphere, where the solar intensity (assuming the sun's emission to remain constant) is uniform from sunrise to sunset. Throughout the dreary winter days, when, even in the country, a leaden sky oppresses us, it is tantalising to reflect that, at the moderate height of 5000 feet, which can be reached by a balloon in a few minutes, there is probably blue sky and brilliant sunshine.

Latitude profoundly, though irregularly, affects air temperature, for in high latitudes less solar heat falls upon each square foot of the earth's surface, and therefore the air resting upon that surface is warmed to a less extent. But obliquity of the sun's rays has no such influence on solar intensity, for the highest readings of solar heat at or near sea-level have been observed near to the Arctic circle, as is seen from the following table:—

Solar Intensity in different Latitudes.

Station.	Latitude.	Sun's Altitude.	Sun Temperature.	Solar Intensity.
	°	°	° C.	° C.
At Sea	0	84	78·9	41·7
Oatlands Park	52 N.	61	75·0	45·0
Isle of Wight	51 „	58	72·3	42·3
At Sea	23 „	56	71·7	45·0
Cassel	51 „	53	68·7	—
Tosten Vierod	59 „	52	73·5	—
Whitby	54 „	50	67·8	36·8
Aak, Romsdal	63 „	49	82·5	48·7
At Sea	30 „	48	70·3	43·6
Bellagio	45 „	47	68·3	39·8

These results show that, with an obliquity of only 6°, the sun temperature and solar intensity were respectively only 78·9° and 41·7° C.; whilst, with an obliquity of 41°, they were 82·5° and 48·7° C. *On the equator at noon, with a nearly vertical sun, the solar intensity was actually 7° C. lower than in Romsdal, only 4° S. of the Arctic circle.* On the other hand, air-warmth diminishes, as a rule, with increase of latitude, although, as the following table shows there are some remarkable exceptions, for it was 1° higher in lat. 52° N. with an obliquity of 29°, than in lat. 5° N. with an obliquity of only 12°, and in the high latitude 63°, with an obliquity of 41°, it was only 1° C. in arrear of the air-warmth at the equator with an obliquity of only 6°.

SHADE TEMPERATURE AT OR NEAR NOON AND SEA-LEVEL.

Station.	Latitude.	Sun's Apparent Altitude.	Temperature.
	°	°	° C.
At Sea, April 10 ..	45 S.	37	18·9
„ March 23 ..	31 „	58	26·3
„ „ 22 ..	29 „	60	29·7
„ „ 18 ..	27 „	65	32·5
„ „ 17 ..	23 „	68	32·8
„ „ 16 ..	20 „	71	29·4
„ „ 13 ..	11 „	82	37·2
„ „ 12 ..	10 „	83	37·2
„ „ 11 ..	9 „	85	36·5
„ „ 6 ..	0	84	37·2
„ „ 4 ..	3 N.	81	30·0
„ „ 3 ..	5 „	78	29·4
„ „ 2 ..	8 „	75	31·7
„ Feb. 24 ..	17 „	64	28·0
„ „ 20 ..	21 „	58	28·3
„ „ 19 ..	23 „	56	27·2
„ „ 16 ..	30 „	48	28·9
„ Jan. 27 ..	51 „	21	10·6
Bellagio, Sept. 17 ..	45 „	47	28·5
Oatlands Park, June 8	52 „	61	30·0
Isle of Wight, May 13	51 „	57	28·9
„ „ „ 14	51 „	58	29·0
„ „ „ 15	51 „	58	30·0
Whitby, Aug. 16 ..	54 „	50	32·2
Aak, Romsdal, July 15	63 „	49	36·2

Shortly summarised, therefore, the conditions most favourable for a genial climate—

Depending on solar intensity, are—

1. Great elevation above sea-level.
2. A light-coloured ground and background.
3. Shelter. Reception of direct and reflected rays.
4. A clear sun with white clouds.
5. A clean atmosphere. No dust, smoke, or fog.
6. A minimum of watery vapour in the air.

Depending on air temperature, are—

1. Slight elevation above sea-level.
2. A dark-coloured ground and background.
3. Shelter. Reception of direct and reflected rays.
4. A clear sun with white clouds.
5. A clean atmosphere. No dust, smoke, or fog.
6. A maximum of watery vapour in the air.

Thus whilst there are three conditions common to both categories, the three remaining ones are diametrically opposed to each other.

TOWN CLIMATE.

The climate of towns depends upon the same essential conditions as that of the country, but some of these are more within our own control in towns.

The great evils of our town climate are excessive heat in summer

and cheerless gloom in winter. We suffer less, however, from excessive solar intensity than continental cities between the same parallels of latitude, owing to the very causes which plunge us into a more miserable gloom in winter. Light-coloured walls neither make our streets look cheerful nor feel hot. Such sad colours as brick, stone, stucco, or paint give to our houses are soon changed to a grimy neutral tint, powerless to reflect either solar light or heat.

The darker the colour of the houses, the cooler the streets and the hotter the rooms during sunshine, and *vice versâ.* Whilst the summer climate in our streets and houses is thus, to a considerable extent controllable, that of winter, which depends so much on a clean atmosphere, is still more so. All our towns are nearly at the sea-level, a position favourable for air-, but not for sun-warmth. In our large towns, however, we artificially create an impenetrable barrier to solar radiation by throwing into the air the imperfectly burnt products of bituminous coal.

These products are of three kinds—soot, tar and steam. Every ton of bituminous coal burnt in our grates gives off about 6 cwts. of volatile but condensable products. The less perfect the combustion the more tar and the less steam will be produced. If perfectly burnt without any smoke, then about 9 cwts. of steam, occupying 27,359 cubic feet at 100° C., or 20,024 cubic feet at 0° C. will be sent into the air. Now 33,333 tons of bituminous coal are, on the average, daily consumed in London in winter, giving 667,460,000 cubic feet of steam at 0° C.

This combustion of enormous quantities of bituminous coal acts in the production of town fog in three ways:—1st. By supplying the basis of all fog—condensed watery particles. 2nd. By determining the condensation of atmospheric moisture in the form of fog. 3rd. By coating the fog particles with tar, and thus making them more persistent.

All fogs have for their basis watery particles, and the greater part even of the suspended matters visible in a ray of electric light consists of these particles, for the air becomes nearly clear when it is heated somewhat above 100° C. [Experiment shown]. Everything therefore which increases the proportion of aqueous vapour in town air tends to produce fog. But aqueous vapour alone would probably never produce fog, for it condenses at once to large particles, which rapidly fall as rain. When, however, solid or liquid particles are present in the air, the minute spherules of fog are produced. This was first shown by Messrs. Coulier and Mascart, in 1875, and their results have since been confirmed by Mr. Aitkin. The speaker showed that air filtered through cotton wool, though afterwards saturated with moisture, produced no fog when its temperature was lowered; but as soon as a small quantity of the dusty air of the theatre was admitted fog was immediately formed, whilst, when a little coal smoke was introduced, a dense and more persistent fog was the result.

The fog once formed is rendered more persistent by the coating of tarry matter which it receives from the products of the imperfect combustion of smoky coal. The speaker had made numerous experiments on the retardation of evaporation by films of coal tar. He had found that the evaporation of water in a platinum dish placed in a strong draught of air was retarded in one experiment by 84 per cent. and in another by 78·6 per cent., when a thin film of coal tar was placed on the surfaces. Even by the mere blowing of coal smoke on the surface of the water for a few seconds, the evaporation was retarded by from 77·3 to 81·5 per cent. Drops of water suspended in loops of platinum wire were also found to have their evaporation retarded by coal smoke. Hence arise the so-called dry fogs which have been observed by Mr. Glaisher in balloon ascents, some examples of which are given in the following table:—

FOG IN COMPARATIVELY DRY AIR.

Place of Ascent.	Altitude.	Temperature of Air.	Degree of Humidity.
	feet.	° F.	100 = Saturation.
Wolverhampton	5,922	53·5	61
Crystal Palace	3,698	38·5	62
" "	9,000	32·5	52
" "	1,000	64·7	53
Wolverton	11,000	30·0	68
Woolwich	6,000	44·0	64
"	4,400	42·0	52

Thus the smoke of our domestic fires constitutes a potent cause both for the generation and the persistency of town fogs. In London, at all events, if all manufacturing operations were absolutely to cease, the fogs would not be perceptibly less dense or irritating. Granting then this cause of town fogs, what are the remedies open to us? The speaker was of opinion that the substitution of a sufficient number of smoke-consuming grates (assuming a smoke-consuming grate to have been invented), for the 1,800,000 fire-places of London was quite hopeless, and that one remedy only could be of any appreciable service—*the importation of bituminous coal must be forbidden.* This is a case in which individual effort can do nothing; but State or municipal action would be simple and decisive.

There need be no fear that the price of smokeless fuel would rise inordinately, for the sources of this fuel are too numerous and inexhaustible to admit of either a monopoly or a serious rise in price. In addition to the enormous stores of smokeless coal in the Welsh coal-fields, every bituminous coal yields a smokeless coke, either in the retorts of gasworks or in coke ovens. On the average, 100 tons of smoky coal yield 60 tons of coke, the remaining 40 tons being driven off as combustible gas, ammoniacal liquor and tar; and as there is an almost unlimited demand for these products, it is not

unlikely that they would, under the circumstances contemplated, repay the cost of coking, and it is worthy of note that coal of very inferior quality makes fairly good coke.

The only objection to the domestic use of smokeless coal and coke is the difficulty of lighting the fire, but this is obviated by the use of gas as proposed by Dr. Siemens. In ordinary grates, however, there is little difficulty in lighting and burning these smokeless fuels if the throat of the chimney be contracted so as to increase the draught. In this way nearly every grate in London could be rendered smokeless at an expenditure of a couple of shillings.

It is unnecessary to enumerate the many advantages of a smokeless atmosphere, but it may here be mentioned that London fogs not only seriously injure health but annually destroy the lives of thousands. In one week alone upwards of 1100 lives have been thus sacrificed in London. We have doubtless still long to wait before the only remedy for London fogs will be adopted; but in the meantime, immunity from their effects, so far as the respiratory organs are concerned, may be obtained by the use of a small and very portable cotton-wool respirator which is made, in accordance with the speaker's directions, by Mr. Casella, of Holborn. [Respirator exhibited.] Armed with this little instrument, he had often passed through the densest and most irritating fogs with perfect immunity, breathing, in fact, all the time, air even purer than that of the country. Such a remedy is, however, obviously of extremely limited application.

In conclusion he said, though we may, with justice, complain of the scanty share of sunshine now received by us, let us not forget that, in our coal-fields, we are compensated by vast stores of the sunlight of past ages. How far, through electricity, these stores can be evoked to supplement the present defective supply, he would be a bold man who would venture to predict. Let us not, however, continue to use this great legacy of light of the past to obscure the small one of the present.

[E. F.]

WEEKLY EVENING MEETING,

Friday, February 17, 1882.

Sir Frederick Bramwell, F.R.S. Vice-President, in the Chair.

Professor John G. McKendrick, M.D. F.R.S.E.

The Breathing of Fishes.

(Abstract deferred.)

WEEKLY EVENING MEETING,

Friday, February 24, 1882.

Sir Frederick Bramwell, F.R.S. Vice-President, in the Chair.

Professor Odling, M.A. F.R.S. *M.R.I.*

Sir B. C. Brodie's Researches on Chemical Allotropy.

(Abstract deferred.)

WEEKLY EVENING MEETING,

Friday, March 3, 1882.

William Bowman, Esq. LL.D. F.R.S. Honorary Secretary and Vice-President, in the Chair.

Alfred Tylor, Esq. F.G.S. *M.R.I.*

Roman Antiquities recently found in London.

The speaker began by referring to some Roman remains discovered near Warwick Square, E.C. London, last year, about nineteen feet below the present surface; and with remarks on a series of diagrams illustrating the history of Roman London, and its site, boundaries, walls, and streets, and the principal roads issuing from it to other parts of the island.

Many specimens of the relics discovered and large drawings of others were exhibited. They will go to the British Museum, on loan.

The collection includes several cinerary urns, containing the results of the cremation of human bodies. One urn, fifteen inches high, was of glass. A remarkable turned vase of stone was found. Four of the urns were inclosed in leaden ossuariæ, made without solder; some of the remainder were protected by roofing tiles.

On the inside of one ossuarium was an emblem of Mithra, the Persian sun-god. The lecturer explained its difference from the emblem chosen by the Emperor Constantine. It differs from the early Christian labarum in being an eight-rayed cross without the R.

In reference to the ossuariæ, Mr. Tylor said that the arts of smelting and working lead were practised and probably invented in this country in very ancient times; and that at Avignon and Lyons he saw Roman lead-work, bearing the inscription "Cantius," i. e. "a Kentishman." Another British word, Cunobarrus, was found on lead-work at Caistor near Peterboro'. This specimen is now in the British Museum.

The coins found during Mr. Tylor's excavations were dated from A.D. 46 to 300. The date of the Mithraic emblem was considered to be soon after A.D. 50, much earlier than that on the Portland Vase.

Suggestive remarks were made on the probably advanced stage of civilisation in Britain at the time of the Roman invasion indicated by the statements of contemporary historians and other sources.

In conclusion, Mr. Tylor stated that the greatest care had been taken to lay down the exact position of each article found, on a plan, and that this would be published in the 'Archælogia' shortly.

GENERAL MONTHLY MEETING,

Monday, March 6, 1882.

GEORGE BUSK, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Walter H. Coffin, Esq. F.L.S. F.C.S.
Andrew Ainslie Common, Esq. F.R.A.S.
Duncan Darroch, Esq.
Captain Montagu Dettmar,
Francis Y. Edgeworth, Esq. M.A.
Mrs. John Macnaught,
Vice-Admiral Frederick Augustus Maxse,
M. de Meritens,
Wilson Noble, Esq. M.A.
George William Stevens, Esq.
Frederick Purdy, Esq. F.S.S.
Frederick Ramadge, Esq.
Mrs. George J. Romanes,

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to M. Janssen for his Photographs of the Sun.

The Chairman reported that he had received the following Letter from Mr. Warren De La Rue:—

"73, PORTLAND PLACE, W.
February 7, 1882.

"DEAR MR. BUSK,

"I am deeply sensible of the indulgent appreciation of my services as Secretary of the Royal Institution, expressed in the Resolution of the Committee of Managers on December 16th, 1881, and adopted by the General Meeting of Members yesterday.

"The kind and sympathetic feeling which pervades the elegantly expressed document, in which the sentiments of the Managers are embodied, is most gratifying to me and my family.

"On my part I can assure you that no post I have ever held has been productive of so much satisfaction and pleasure as that which I most reluctantly resigned on account of failing health.

"To the Royal Institution I owe a debt of deep gratitude for the help I have received in and the impulse it has given to my scientific work, from the time, now far distant, when Faraday first conferred on me the boon of a Friday Evening card.

"My interest in the Royal Institution will never cease.

"Yours sincerely,
"WARREN DE LA RUE."

The following arrangements for the Lectures after Easter were announced:—

Edward B. Tylor, Esq. D.C.L. F.R.S.—Four Lectures on The History of Customs and Beliefs; on Tuesdays, April 18 to May 9.

Professor Arthur Gamgee, M.D. F.R.S.—Four Lectures on Digestion; on Tuesdays, May 16 to June 6.

Professor Dewar, M.A. F.R.S.—Eight Lectures on The Chemical and Physical Properties of the Metals; on Thursdays, April 20 to June 8.

Frederick Pollock, Esq. M.A.—Four Lectures on The History of the Science of Politics; on Saturdays, April 22 to May 13.

David Masson, Esq. LL.D. F.R.S.E. Professor of Rhetoric and English Literature, University of Edinburgh.—Four Lectures on Poetry and its Literary Forms; on Saturdays, May 20 to June 10.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Secretary of State for India—Report on Public Instruction in Bengal, 1880–1. fol. 1881.
Accademia dei Lincei, Reale, Roma—Atti, Serie Terza: Vol. VI. Fasc. 6. 4to. 1882.
Astronomical Society, Royal—Monthly Notices, Vol. XLII. No. 3. 8vo. 1882.
Memoirs, Vol. XLVI. 1880–1. 4to. 1881.
Bankers' Institute—Journal, Vol. III. Part 2. 8vo. 1882.
Bavarian Academy of Sciences, Royal—Sitzungsberichte: 1882, Heft 1. 8vo.
British Architects, Royal Institute of—Proceedings, 1881–2, Nos. 9, 10. 4to. 1881–2.
British Museum Trustees—Catalogue of Spanish MSS. By P. de Gazangos. Vol. III. 8vo. 1881.
Chemical Society—Journal for Feb. 1882. 8vo.
Civil Engineers' Institution—Proceedings, 1881–2. 8vo. Nos. 7–9.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Series II. Vol. II. Part 1. 8vo. 1882.
Crookes, W. Odling, W. and C. Meymott Tidy (the Authors)—Reports on London Water Supply, No. 13. 4to. 1882.
Editors—American Journal of Science for Feb. 1882. 8vo.
Analyst for Feb. 1882. 8vo.
Athenæum for Feb. 1882. 4to.
Chemical News for Feb. 1882. 4to.
Engineer for Feb. 1882. fol.
Horological Journal for Feb. 1882. 8vo.
Iron for Feb. 1882. 4to.
Nature for Feb. 1882. 4to.
Revue Scientifique and Revue Politique et Littéraire for Feb. 1882. 4to.
Telegraphic Journal for Feb. 1882. fol.
Franklin Institute—Journal, No. 674. 8vo. 1882.
Geographical Society, Royal—Proceedings, New Series, Vol. IV. No. 3. 8vo. 1882.
Geological Institute, Imperial, Vienna—Verhandlungen, 1881, Nos. 1–18. 8vo.
Jahrbuch: Band XXXI. No. 4. 8vo. 1881.
Geological Society—Quarterly Journal, No. 149. 8vo. 1882.
Abstracts of Proceedings, 1881–2, Nos. 414–416. 8vo.
Grey, Henry, Esq. (the Author)—The Classics for the Million: an Epitome, in English, of the Works of Greek and Latin Authors. 12mo. 1881.
Lisbon, Sociedade de Geographia—Boletim, 2ª Serie, Nos. 7 and 8. 8vo. 1881.

Manchester Geological Society—Transactions, Vol. XVI. Parts 11, 12. 8vo. 1881.
Meteorological Office—Communications from the International Polar Commission. Part 1. 4to. St. Petersburg, 1882.
Miller, W. J. C. Esq. B.A. (the Registrar)—The Medical Register. 8vo. 1882.
The Dentist's Register. 8vo. 1882.
Pharmaceutical Society of Great Britain—Journal, Feb. 1882. 8vo.
Calendar, 1882. 8vo. 1882.
Photographic Society—Journal, New Series, Vol. VI. No. 5. 8vo. 1882.
Pole, William, Esq. F.R.S. M.Inst.C.E. (the Author)—A Study of the Problem of Aerial Navigation. (Min. of Proc. of Inst. of Civil Eng. vol. 67.) 8vo. 1882.
Preussische Akademie der Wissenschaften—Monatsberichte: Dec. 1881. 8vo.
Royal Dublin Society—Scientific Transactions, Vol. I. (Series II.) Parts 13, 14. 4to. 1880–1.
Scientific Proceedings, Vol. II. (New Series) Part 7. Vol. III. Parts 1–4. 8vo. 1880–1.
Royal Society of London—Proceedings, No. 217. 8vo. 1882.
Society of Arts—Journal, Feb. 1882. 8vo.
Statham, H. H. Esq. (the Author)—Notes on Ornament. (Lectures delivered at the Royal Institution.) (Portfolio, Feb. 1882.)
St. Bartholomew's Hospital—Reports, Vol. XVII. 8vo. 1881.
St. Pétersbourg, Akadémie des Sciences—Memoires, 7e Série, Tome XXIX. No. 2. 4to. 1881.
Bulletins, Tome XXVII. No. 4. 4to. 1881.
Symons, G. J.—Monthly Meteorological Magazine, Feb. 1882. 8vo.
Telegraph Engineers, Society of—Vol. X. No. 39. 8vo. 1882.
Teyler Museum—Archives: Série II. 2e Partie. 4to. 1881.
Origine et but de la Fondation Teyler. Par E. van der Ven. 4to. 1881.
Tokio University—Memoirs, Nos. 4, 5. 4to. 1881.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1882: No. 1. 4to.

WEEKLY EVENING MEETING,

Friday, March 10, 1882.

SIR FREDERICK BRAMWELL, F.R.S. Vice-President, in the Chair.

JOSEPH W. SWAN, Esq.

Electric Lighting by Incandescence.

SPEAKING in this place on electric light, I can neither forget, nor forbear to mention, as inseparably associated with the subject and with the Royal Institution, the familiar, illustrious, names of Davy and Faraday. It was in connection with this Institution that, eighty years ago, the first electric light experiments were made by Davy, and it was also in connection with this Institution that, forty years later, the foundations of the methods, by means of which electric lighting has been made useful, were strongly laid by Faraday.

I do not propose to describe at any length the method of Davy. I must, however, describe it slightly, if only to make clear the difference between it and the newer method which I wish more particularly to bring under your notice.

The method of Davy consists, as almost all of you know, in producing electrically a stream of white-hot gas between two pieces of carbon.

When electric light is produced in this manner, the conditions, which surround the process are such as render it impossible to obtain a small light with proportionally small expenditure of power. In order to sustain the arc in a state approaching stability, a high electro-motive force and a strong current are necessary; in fact, such electro-motive force and such current as correspond to the production of a luminous centre of at least several hundred candle-power. When an attempt is made to produce a smaller centre of light by the employment of a proportionally small amount of electrical energy, the mechanical difficulties of maintaining a stable arc, and the diminution in the amount of light (far beyond the diminished power employed), put a stop to reduction at a point at which much too large a light is produced for common purposes.

The often-repeated question, "Will electricity supersede gas?" could be promptly answered if we were confined to this method of producing electric light; and for the simple reason that it is impossible, by this method, to produce individual lights of moderate power.

The electric arc does very well for street lighting, as you all know from what is to be seen in the City. It also does very well for

the illumination of such large enclosed spaces as railway stations; but it is totally unsuited for domestic lighting, and for nine-tenths of the other purposes for which artificial light is required. If electricity is to compete successfully with gas in the general field of artificial lighting, it is necessary to find some other means of obtaining light through its agency than that with which we have hitherto been familiar. Our hope centres in the method—I will not say, the *new* method—but the method which until within the last few years has not been applied with entire success, but which, within a recent period, has been rendered perfectly practicable—I mean the method of producing light *by electrical incandescence.*

The fate of electricity as an agent for the production of artificial light in substitution for gas, depends greatly on the success or non-success of this method; for it is the only one yet discovered which adapts itself with anything like completeness to all the purposes for which artificial lighting is required.

If we are able to produce light *economically* through the medium of *electrical incandescence*, in small quantities, or in large quantities, as it may be required, and at a cost not exceeding the cost of the same amount of gas light, then there can be little doubt—there can, I think, be *no* doubt—that in such a form, electric light has a great future before it. I propose, therefore, to explain the principle of this method of *lighting by incandescence*, to show *how it can be applied*, and to discuss the question of *its cost.*

When an electrical current traverses a conducting wire, a certain amount of *resistance* is opposed to the passage of the current. One of the effects of this conflict of forces is the development of heat. The amount of heat so developed depends on the nature of the wire—on its length and thickness, and on the strength of the current which it carries. If the wire be thin and the current strong, the heat developed in it may be so great as to raise it to a white heat.

The experiment I have just shown, illustrates the principle of Electric Lighting by Incandescence, which is briefly this—that a *state of white heat may be produced in a continuous solid conductor by passing a sufficiently strong electrical current through it.*

A principle, the importance of which cannot well be overestimated, underlies this method of producing light electrically—namely, the principle of *divisibility.* By means of electric incandescence it is possible to produce exceedingly small centres of light, even so small as the light of a single candle; and with no greater expenditure of power, in proportion to the light produced, than is involved in the maintenance of light-centres 10 or 100 times greater. Given a certain kind of wire, for example a platinum wire, the 100th of an inch in diameter, a certain quantity of current would make this wire white-hot whatever its length. If in one case the wire were one inch long and in another case ten inches long, the same current passing through these two pieces of similar wire, would heat both to precisely the same temperature. But in order to force the same current through

the ten times longer piece, ten times the electro-motive force or, if I may be allowed the expression, electrical pressure, is required, and exactly ten times the amount of energy would be expended in producing this increased electro-motive force.

Considering, therefore, the proportion between power applied and light produced, there is neither gain nor loss in heating these different lengths of wire. In the case of the longer wire, as it had ten times the extent of surface, ten times more light was radiated from it than from the shorter wire, and that is exactly equivalent to the proportional amount of power absorbed. It is therefore evident that *whether a short piece of wire or a long piece is electrically heated, the amount of light produced is exactly proportional to the power expended in producing it.*

This is extremely important: for not only does it make it possible to produce a small light where a small light is required, without having to pay for it at a higher rate than for a larger light, but it gives also the great advantage of obtaining *equal distribution* of light. As the illuminating effect of light is inversely as the square of the distance of its source, it follows that where a large space is to be lighted, if the lighting is accomplished by means of centres of light of great power, a much larger total quantity of light has to be employed, in order to make the spaces remotest from these centres sufficiently light, than would be required if the illumination of the space were obtained by numerous smaller lights equally distributed.

In order to practically apply the principle of producing light by the incandescence of an electrically heated continuous solid conductor, it is necessary to select for the light-giving body a material which offers a considerable *resistance* to the passage of the electric current, and which is also capable of bearing an exceedingly high temperature without undergoing fusion or other change.

As an illustration of the difference that exists among different substances in respect of *resistance* to the flow of an electric current, and consequent tendency to become heated in the act of electrical transmission, here is a wire formed in alternate sections of platinum and silver; the wire is perfectly uniform in diameter, and when I pass an electric current through it, although the current is uniform in every part, yet, as you see, the wire is not uniformly hot, but white-hot only in parts. The white-hot sections are platinum, the dark sections are silver. Platinum offers a higher degree of resistance to the passage of the electric current than silver, and in consequence of this, more heat is developed in the platinum than in the silver sections.

The high electrical resistance of platinum, and its high melting-point, mark it out as one of the most likely of the metals to be useful in the construction of incandescent lamps. When platinum is mixed with 10 or 20 per cent. of iridium, an alloy is formed, which has a much higher melting-point than platinum; and many attempts have been made to employ this alloy in electric lamps. But these attempts have not been successful, chiefly because high as is the melting-point

D 2

of iridio-platinum, it is not high enough to allow of its being heated to a degree that would yield a sufficiently large return in light for energy expended. Before an economical temperature is reached, iridio-platinum wire slowly volatilises and breaks. This is a fatal fault, because *in obtaining light by incandescence there is the greatest imaginable advantage in being able to heat the incandescing body to an extremely high temperature.* I will illustrate this by experiment.

Here is a glass bulb containing a filament of carbon. When I pass through the filament *one unit* of current, light equal to *two candles* is produced. If now I increase the current by *one half*, making it *one unit and a half*, the light is increased to *thirty candles*, or thereabout, so that for this one half increase of current (which involves nearly a *doubling of the energy* expended), *fifteen times more light* is produced.

It will readily be understood from what I have shown that it is essential to economy that the incandescing material should be able to bear an enormous temperature without fusion. We know of no metal that fulfils this requirement; but there is a non-metallic substance which does so in an eminent degree, and which also possesses another necessary quality, that of *low conductivity.* The substance is carbon. In attempting to utilise carbon for the purpose in question, there are several serious practical difficulties to be overcome. There is, in the first place, the mechanical difficulty arising from its intractability. Carbon, as we commonly know it, is a brittle and non-elastic substance, possessing neither ductility nor plasticity to favour its being shaped suitably for use in an electric lamp. Yet, in order to render it serviceable for this purpose, it is necessary to form it into a slender filament, which must possess sufficient strength and elasticity to allow of its being firmly attached to conducting wires, and to prevent its breaking. If heated white hot in the air, carbon burns away; and therefore means must be found for preventing its combustion. It must either be placed in an atmosphere of some inert gas or in a vacuum.

During the last forty years, spasmodic efforts have from time to time been made to grapple with the many difficulties which surround the use of carbon as the wick of an electric lamp. It is only within the last three or four years that these difficulties can be said to have been surmounted. It is now found that carbon can be produced in the form of straight or bent filaments of extreme thinness, and possessing a great degree of elasticity and strength. Such filaments can be produced in various ways—by the carbonisation of paper, thread, and fibrous woods and grasses. Excellent carbon filaments can be produced from the bamboo, and also from cotton thread treated with sulphuric acid. The sulphuric acid treatment effects a change in the cotton thread similar to that which is effected in paper in the process of making parchment paper. In carbonising these materials, it is of course necessary to preserve them from contact with the air. This is done by surrounding them with charcoal.

Here is an example of a carbon filament produced from parchmentised cotton thread. The filament is not more than the ·01 of an inch in diameter, and yet a length of three inches, having therefore a surface of nearly the one-tenth of an inch, gives a light of twenty candles when made incandescent to a moderate degree.

I have said, that, in order to preserve these slender carbon filaments from combustion, they must be placed in a vacuum; and experience has shown that if the filaments are to be durable, the vacuum must be exceptionally good. One of the chief causes of failure of the earlier attempts to utilise the incandescence of carbon, was the imperfection of the vacua in which the white-hot filaments were placed; and the success which has recently been obtained is in great measure due to the production of a better vacuum in the lamps.

In the primitive lamps, the glass shade or globe which enclosed the carbon filament was large, and usually had screw joints, with leather or indiarubber washers. The vacuum was made either by filling the lamp with mercury, and then running the mercury out so as to leave a vacuum like that at the upper end of a barometer, or the air was exhausted by a common air pump. The invention of the mercury pump by Dr. Sprengel, and the publication of the delicate and beautiful experiments of Mr. Crookes in connection with the radiometer, revealed the conditions under which a really high vacuum could be produced, and in fact gave quite a new meaning to the word vacuum. It was evident that the old incandescent lamp experiments had not been made under suitable conditions as to vacuum; and that before condemning the use of carbon, its durability in a really high vacuum required still to be tested. This idea having occurred to me, I communicated it to Mr. Stearn, who was working on the subject of high vacua, and asked his co-operation in a course of experiments having for their object to ascertain whether a carbon filament produced by the carbonisation of paper, and made incandescent in a high vacuum was durable. After much experimenting we arrived at the conclusion that *when a well-formed carbon filament is firmly connected with conducting wires, and placed in a hermetically sealed glass ball, perfectly exhausted, the filament suffers no apparent change even when heated to an extreme degree of whiteness.* This result was reached in 1878. It has since then become clearly evident that Mr. Edison had the same idea and reached the same conclusion as Mr. Stearn and myself.

A necessary condition of the higher vacuum was the simplification of the lamp. In its construction there must be as little as possible of *any* material, and there must be none of such material as could occlude gas, which being eventually given out would spoil the vacuum. There must besides be no joints except those made by the glass-blower.

Therefore, naturally and per force of circumstances, the incandescent carbon lamp took the most elementary form, resolving itself into a *simple bulb, pierced by two platinum wires supporting a filament of carbon.* Probably the first lamp, having this elementary character, ever publicly exhibited, was shown in operation at a meeting of the

Literary and Philosophical Society of Newcastle in February, 1879. The vacuum had been produced by Mr. Stearn by means of an improved Sprengel pump of his invention.

Blackening of the lamp glass, and speedy breaking of the carbons, had been such invariable accompaniments of the old conditions of imperfect vacua, and of imperfect contact between carbon and conducting wires, as to have led to the conclusion that the carbon was volatilised. But under the new conditions these faults entirely disappeared; and carefully conducted experiments have shown that well-made lamps are quite serviceable after more than a thousand hours' continual use.

Here are some specimens of the latest and most perfected forms of lamp. The mode of attaching the filament to the conducting wires by means of a tiny tube of platinum, and also the improved form of the lamp, are due to the skill of Mr. Gimmingham.

The lamp is easily attached and detached from the socket which connects it with the conducting wires; and can be adapted to a great variety of fittings, and these may be provided with switches or taps for lighting or extinguishing the lamps. I have here a lamp fitted especially for use in mines. The current may be supplied either through main wires from a dynamo-electrical machine, with flexible branch wires to the lamp, or it may be fed by a set of portable store cells closely connected with it. I will give you an illustration of the *quality* of the light these incandescent lamps are capable of producing by turning the current from a Siemens' dynamo-electric machine (which is working by means of a gas engine in the basement of the building) through sixty lamps ranged round the front of the gallery and through six on the table. (The theatre was now completely illuminated by means of the lamps, the gas being turned off during the rest of the lecture.)

It is evident by the appearance of the flowers on the table that colours are seen very truly by this light, and this is suggestive of its suitability for the lighting of pictures.

The heat produced is comparatively very small; and of course there are no noxious vapours.

And now I may, I think, fairly say that the difficulties encountered in the construction of incandescent electric lamps have been completely conquered, and that their use is *economically practicable.* In making this statement I mean, that, both as regards the *cost of the lamp* itself and *the cost of supplying electricity to illuminate it,* light can be produced at a cost which will compare not unfavourably with the cost of gas light. It is evident that if this opinion can be sustained, lighting by electricity at once assumes a position of the widest public interest, and of the greatest economic importance; and in view of this, I may be permitted to enter with some detail into a consideration of the facts which support it.

There has now been sufficient experience in the manufacture of lamps to leave no doubt that they can be cheaply constructed, and we

know by actual experiment that continuous heating to a fairly high degree of incandescence during 1200 hours does not destroy a well-made lamp. What the utmost limit of a lamp's life may be, we really do not know. Probably it will be an ever-increasing span ; as, with increasing experience, processes of manufacture are sure to become more and more perfect. Taking it, therefore, as fully established that *a cheap and durable lamp can now be made*, the further question is as to *the cost of the means of its illumination.*

This question in its simplest form is that of the more or less economical use of coal; for coal is the principal raw material alike in the production of gas and of electric light. In the one case, the coal is consumed in producing gas which is burnt; in the other in producing motive power, and, by its means, electricity.

The cost of producing light by means of electric incandescence may be compared with the cost of producing gas light in this way,—2 cwt. of coal produces 1000 cubic feet of gas, and this quantity of gas, of the quality called fifteen-candle gas, will produce 3000 candle-light for one hour. But besides the product of gas, the coal yields certain bye products of almost equal value. I will, therefore, take it that we have, in effect, 1000 feet of gas from one cwt. of coal instead of from two, as is actually the case.

And now, as regards the production of electricity. One cwt. of coal—that is the same measure *in point of value* as gives 1000 feet of gas—will give 50 horse-power for one hour. Repeated and reliable experiments show that we can obtain through the medium of incandescent lamps at least 200 candle-light per horse-power per hour. But as there is waste in the conversion of motive power into electricity, and also in the conducting wires, let us make a liberal deduction of 25 per cent., and take only 150 candle-light as the nett available product of 1 horse-power; then for 50 horse-power (the product of 1 cwt. of coal), we have 7500 candle-light, as against 3000 candle-light from *an equivalent value* of gas. That is to say, two and a half times more light.

There still remains an allowance to be made to cover the cost of the renewal of lamps. There is a parallel expense in connection with gas lighting in the cost of the renewal of gas-burners, gas globes, gas chimnies, &c. I cannot say that I think these charges against gas lighting will equal the corresponding charges against electric lighting, unless we import into the account—as I think it right to do—the consideration that, without a good deal of expense be incurred in the renewal of burners, and unless minute attention be given, far beyond what is actually given, to all the conditions under which the gas is burned, nothing like the full light product which I have allowed to be obtainable from the burning of 1000 cubic feet of gas, will be obtained, and, as a matter of fact, is not commonly obtained, especially in domestic lighting. Taking this into account, and considering what would have to be done to obtain the full yield of light from gas and that if it be not done, then the estimate I have

made is too favourable, I think but little, if any, greater allowance need be made for the charge in connection with the renewal of lamps in electric lighting than ought to be made for the corresponding charges for the renewal of gas-burners, globes, chimneys, &c. But it will be seen that even if the cost for renewal of lamps should prove to be considerably greater than the corresponding expense in the case of gas, there is a wide margin to meet them before we have reached the limit of the cost of gas lighting.

I think too it must be fairly taken into account and placed to the credit of electric lighting, that by this mode of lighting there is entire avoidance of the damage to furnishings and decorations of houses, to books, pictures, and to goods in shops, which is caused through lighting by gas, and which entails a large expenditure for repair, and a large amount of loss which is irreparable.

I have based these computations of cost of electric light on the supposition that the light product of 1 horse-power is 150 candles. But if durability of the lamps had not to be considered, and it were an abstract question how much light can be obtained through the medium of an incandescent filament of carbon, then one might, without deviating from ascertained fact, have spoken of a very much larger amount of light as obtainable by this expenditure of motive power. I might have assumed double or even more than double the light for this expenditure. Certainly double and treble the result I have supposed can actually be obtained. The figures I have taken are those which consist with long life to the lamps. If we take more light for a given expenditure of power, we shall have to renew the lamps oftener, and so what we gain in one way we lose in another. But it is extremely probable that a higher degree of incandescence than that on which I have based my calculations of cost, may prove to be compatible with durability of the lamps. In that case, the economy of electric lighting will be greater than I have stated.

In comparing the cost of producing light by gas and by electricity, I have only dealt with the radical item of coal in both cases. Gas lighting is entirely dependent upon coal—electric lighting is not, but in all probability coal will be the chief source of energy in the case of electric lighting also. When, however, water power is available, electric lighting is in a position of still greater advantage, and, in point of cost, altogether beyond comparison with other means of producing light.

To complete the comparison between the cost of electric light and gas light, we must consider not only the amount of coal required to yield a certain product of light in the one case and in the other, but also the cost of converting the coal into electric current and into gas; that is to say, THE COST OF MANUFACTURE of electricity and THE COST OF MANUFACTURE of gas. I cannot speak with the same exactness of detail on this point, as I did on the comparative cost of the raw material. But if you consider the nature of the process of gas manufacture, and that it is a process, in so far as the lifting of coal by

manual labour is concerned, not very unlike the stoking of a steam boiler, and if electricity is generated by means of steam, then the manual labour chiefly involved in both processes is not unlike. It is evident that in gas manufacture it would be necessary to shovel into the furnaces and retorts five or six times as much coal to yield the same light product as would be obtainable through the steam engine and incandescent lamps. But here again it is necessary to allow for the value of the labour in connection with the products other than gas, and hence it is right to cut down the difference I have mentioned to half—i.e. debit gas with only half the cost of manufacture, in the same way as in our calculation we have charged gas with only one-half the coal actually used. But when that is done, there is still a difference of probably three to one in respect of labour in favour of electric lighting.

I have made these large allowances of material and labour in favour of the cost of gas, but it is well known that the bye products are but rarely of the value I have assumed. I desire, however, to allow all that can be claimed for gas.

With regard to the COST OF PLANT, I think there will be a more even balance in the two cases. In a gasworks you have retorts and furnaces, purifying chambers and gasometers, engines, boilers, and appliances for distributing the gas and regulating its pressure. Plant for generating electricity on a large scale would consist principally of boilers, steam engines, dynamo-electric machines, and batteries for storage.

No such electrical station, on the scale and in the complete form I am supposing, has yet been put into actual operation; but several small stations for the manufacture of electricity already exist in England, and a large station designed by Mr. Edison is, if I am rightly informed, almost completed in America. We are therefore on the point of ascertaining by actual experience, what the *cost of the works* for generating electricity will be. Meanwhile, we know precisely the cost of boilers and engines, and we know approximately what ought to be the cost of dynamo-electric machines of suitably large size. We have, therefore, sufficient grounds for concluding that to produce a given quantity of light electrically the cost of plant would not exceed greatly, if at all, the cost of equivalent gas plant.

There remains to be considered, in connection with this part of the subject, the *cost of distribution.* Can electricity be distributed as widely and cheaply as gas? On one condition, which I fully hope can be complied with, this may be answered in the affirmative. The condition is that it be found practicable and safe to distribute electricity of comparatively high tension.

The importance of this condition will be understood when it is remembered that to effectively utilise electricity in the production of light in the manner I have been explaining, it is necessary that the *resistance in the carbon of the lamps* should be relatively great to the

resistance in the wires which convey the current to them. When lamps are so united with the conducting wire, that the current which it conveys is divided amongst them, you have a condition of things in which the aggregate resistance of the lamps will be very small, and the conducting wire, to have a relatively small resistance, must either be *very short*, or, if it be long, it must be *very thick*, otherwise there will be excessive waste of energy; in fact, it will not be a practical condition of things.

In order to supply the current to the lamps economically, there should be comparatively little resistance in the line. A waste of energy through the resistance of the wire of 10 or perhaps 20 per cent. might be allowable, but if the current is supplied to the lamps in the manner I have described—that of *multiple arc*, each lamp being as it were *a crossing between two main wires*, then—and even if the individual lamps offered a somewhat higher degree of resistance than the lamps now in actual use—the thickness of the conductor would become excessive if the line was far extended. In a line of half a mile, for instance, the weight of copper in the conductor would become so great, in proportion to the number of lamps supplied through it, as to be a serious charge on the light. On the other hand, if a smaller conducting wire were used, the waste of energy and consequent cost would greatly exceed that I have mentioned as the permissive limit.

Distribution in this manner has the merit of simplicity, it involves no danger to life from accidental shock; and it does not demand great care in the insulation of the conductor. But it has the great defect of limiting within comparatively small bounds the area over which the power for lighting could be distributed from one centre. In order to light a large town electrically on this system, it would be necessary to have a number of supply stations, perhaps half a mile or a mile apart. It is evidently desirable to be able to effect a wider distribution than this, and I hope that either by arranging the lamps *in series*, so that the same current passes through several lamps in succession, or by means of *secondary voltaic cells*, placed as electric reservoirs in each house, it may be possible to economically obtain a much wider distribution.

Whether by the method of multiple arc (illustrated by Diagram I.) which necessitates the multiplication of electrical stations; or by means of the simple series (illustrated by Diagram II.), or by means of secondary batteries connected with each other from house to house in single series, the lamps being fed from these in multiple arc (as illustrated by Diagram III.), I am quite satisfied that comparatively with the distribution of gas, the distribution of electricity is sufficiently economical to permit of its practical application on a large scale.

As to the cost of laying wires in a house, I have it on the authority of Sir Wm. Thomson, who has just had his house completely fitted with incandescent lamps from attics to cellars—to the

entire banishment of gas—that the cost of internal wires for the electric lamps is less than the cost of plumbing in connection with gas pipes.

I have expended an amount of time on the question of *cost* which I fear must have been tedious; but I have done so from the conviction that the practical interest of the matter depends on this point. If electric lighting by incandescence is not an economical process, it is unimportant; but if it can be established—and I have no doubt that it can—that this mode of producing light is economical, the subject assumes an aspect of the greatest importance.

Although at the present moment there may be deficiencies in the apparatus for generating and storing electricity on a very large scale, and but little experience in distributing it for lighting purposes over wide areas, and consequently much yet to be learnt in these respects; yet, if once it can be clearly established that light for light, electricity is as cheap as gas, and that it can be made applicable to all the purposes for which artificial light is required, electric light possesses such marked advantages in connection with health, with the preservation of property, and in respect of safety, as to leave it as nearly certain as anything in this world can be, that the wide substitution of the one form of light for the other is only a question of time.

[J. W. S.]

EXTRA EVENING MEETING,

Monday, March 13, 1882.

H.R.H. The Prince of Wales, K.G. F.R.S. Vice-Patron and Honorary Member, in the Chair.

Eadweard Muybridge, of San Francisco.

The Attitudes of Animals in Motion, illustrated with the Zoopraxiscope.

The problem of animal mechanism has engaged the attention of mankind during the entire period of the world's history.

Job describes the action of the horse; Homer, that of the ox; it engaged the profound attention of Aristotle, and Borelli devoted a lifetime to its attempted solution. In every age, and in every country, philosophers have found it a subject of exhaustless research. Marey, the eminent French savant of our own day, dissatisfied with the investigations of his predecessors, and with the object of obtaining more accurate information than their works afforded him, employed a system of flexible tubes, connected at one end with elastic air-chambers, which were attached to the shoes of a horse; and at the other end with some mechanism, held in the hand of the animal's rider. The alternate compression and expansion of the air in the chambers caused pencils to record upon a revolving cylinder the successive or simultaneous action of each foot, as it correspondingly rested upon or was raised from the ground. By this original and ingenious method, much interesting and valuable information was obtained, and new light thrown upon movements until then but imperfectly understood.

While the philosopher was exhausting his endeavours to expound the laws that control, and the elements that effect the movements associated with animal life, the artist, with a few exceptions, seems to have been content with the observations of his earliest predecessors in design, and to have accepted as authentic without further inquiry, the pictorial and sculptural representations of moving animals bequeathed from the remote ages of tradition.

When the body of an animal is being carried forward with uniform motion, the limbs in their relations to it have alternately a progressive and a retrogressive action, their various portions accelerating in comparative speed and repose as they extend downwards to the feet, which are subjected to successive changes from a condition of absolute rest, to a varying increased velocity in comparison with that of the body.

The action of no single limb can be availed of for artistic purposes without a knowledge of the synchronous action of the other limbs;

and to the extreme difficulty, almost impossibility, of the mind being capable of appreciating the simultaneous motion of the four limbs of an animal, even in the slower movements, may be attributed the innumerable errors into which investigators by observation have been betrayed. When these synchronous movements and the successive attitudes they occasion are understood, we at once see the simplicity of animal locomotion, in all its various types and alternations. The walk of a quadruped being its slowest progressive movement would seem to be a very simple action, easy of observation and presenting but little difficulty for analysis, yet it has occasioned interminable controversies among the closest and most experienced observers.

When, during a gallop, the fore and hind legs are severally and consecutively thrust forwards and backwards to their fullest extent, their comparative inaction may create in the mind of the careless observer an impression of indistinct outlines; these successive appearances were probably combined by the earliest sculptors and painters, and with grotesque exaggeration adopted as the solitary position to illustrate great speed. Or, as is very likely, excessive projection of limb was intended to symbolise speed, just as excess in size was an indication of rank. This opinion is to some extent corroborated by the productions of the Grecian artists in their best period, when their heroes are represented of the same size as other men, and their horses in attitudes more nearly resembling those possible for them to assume. The remarkable conventional attitude of the Egyptians, however, has, with few modifications, been used by artists of nearly every age to represent the action of galloping, and prevails without recognised correction in all civilised countries at the present day.

The ambition and perhaps also the province of art in its most exalted sense, is to be a delineator of impressions, a creator of effects, rather than a recorder of facts. Whether in the illustrations of the attitudes of animals in motion the artist is justified in sacrificing truth, for an impression so vague as to be dispelled by the first studied observation, is a question perhaps as much a subject of controversy now as it was in the time of Lysippus, who ridiculed other sculptors for making men as they existed in nature; boasting that he himself made them as they ought to be.

A few eminent artists, notable among whom is Meissonier, have endeavoured in depicting the slower movements of animals to invoke the aid of truth instead of imagination to direct their pencil, but with little encouragement from their critics; until recently, however, artists and critics alike have necessarily had to depend upon their observation alone to justify their conceptions or to support their theories.

Photography, at first regarded as a curiosity of science, was soon recognised as a most important factor in the search for truth, and its more popular use is now entirely subordinated by its value to the astronomer, the anatomist, the pathologist, and other investigators of

the complex problems of nature. The artist, however, still hesitates to avail himself of the resources of what may be at least acknowledged as a handmaiden of art, if not admitted to its most exalted ranks.

Having devoted much attention in California to experiments in instantaneous photography, I, in 1872, at the suggestion of the editor of a San Francisco newspaper, obtained a few photographic impressions of a horse during a fast trot.

At this time much controversy prevailed among experienced horsemen as to whether all the feet of a horse while trotting were entirely clear of the ground at the same instant of time. A few experiments made in that year proved a fact which should have been self-evident.

Being much interested with the experiments of Professor Marey, in 1877 I invented a method for the employment of a number of photographic cameras, arranged in a line parallel to a track over which the animal would be caused to move, with the object of obtaining, at regulated intervals of time or distance, several consecutive impressions of him during a single complete stride as he passed along in front of the cameras, and so of more completely investigating the successive attitudes of animals while in motion than could be accomplished by the system of M. Marey.

I explained the plan of my intended experiments to a wealthy resident of San Francisco—Mr. Stanford—who liberally agreed to place the resources of his stock-breeding farm at my disposal, and to reimburse the expenses of my investigations, upon condition of my supplying him, for his private use, with a few copies of the contemplated results. The apparatus used and its arrangement will be better understood by a reference to the accompanying drawings.

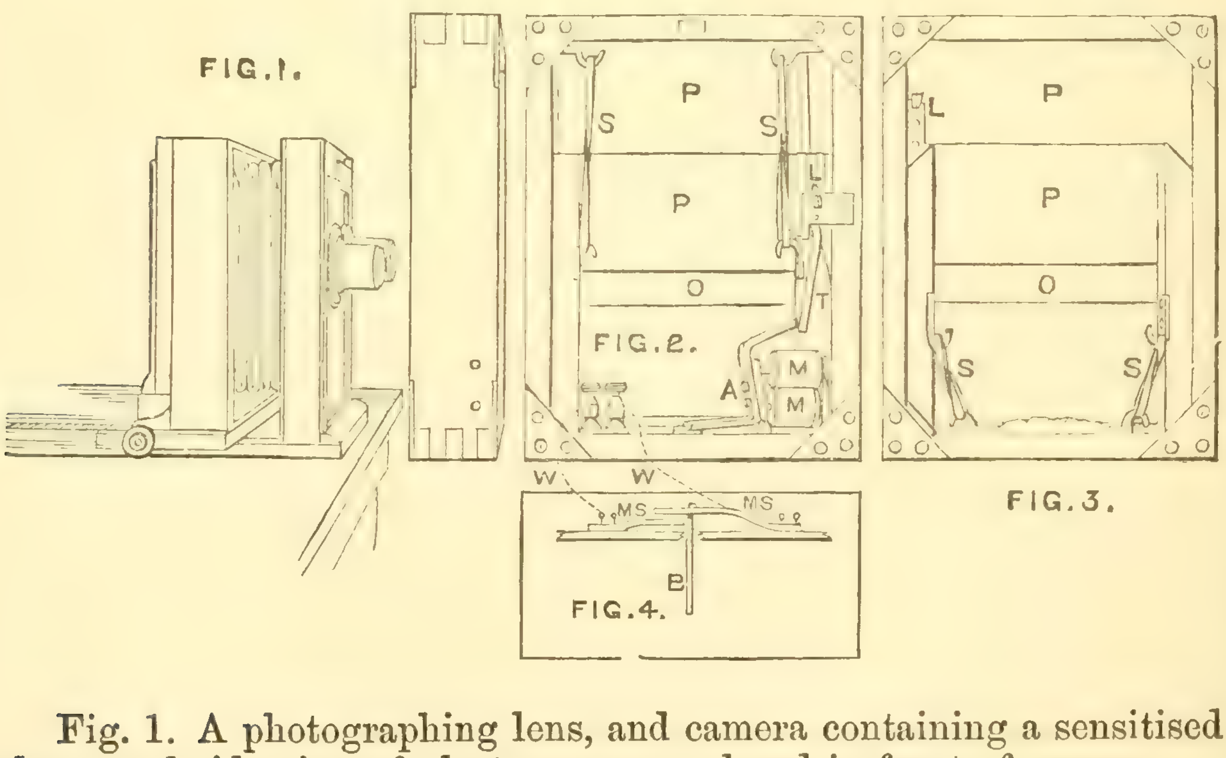


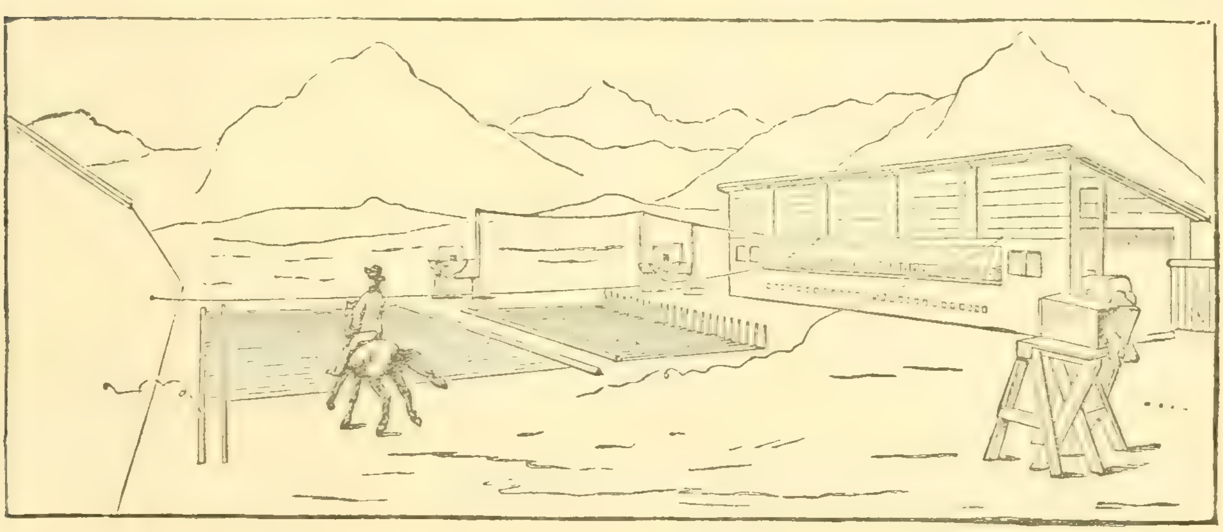
Fig. 1. A photographing lens, and camera containing a sensitised plate; and side view of electro-exposor placed in front of camera.

Fig. 2. Back view of electro-exposor. Two shutters P P, each comprising two panels, with an opening O between them, are adjusted to move freely up and down in a frame; they are here arranged ready for an exposure, and are held in position by a latch L and trigger T, all light being excluded from the lens. A slight extra tension of the thread B, Fig. 4, will cause a contact of the metal springs M S, and complete a circuit of electricity through the wires W W and the electro-magnet M; the consequent attraction causes the armature A to strike the trigger, the latch is released, the shutters are drawn respectively upwards and downwards by means of the rubber springs S S, and light is admitted to the sensitised plate while the openings in the shutters are passing each other in front of the lens.

Fig. 3. Front view of electro-exposor after exposure of the plate.

Fig. 5. General view of studio, operating track, and background. In the studio are arranged 24 photographing cameras at a distance of 12 inches from the centre of each lens; an electro-exposor is securely fixed in front of each camera. Threads 12 inches apart are stretched across the track (only two of which are introduced in the

FIG. 5.



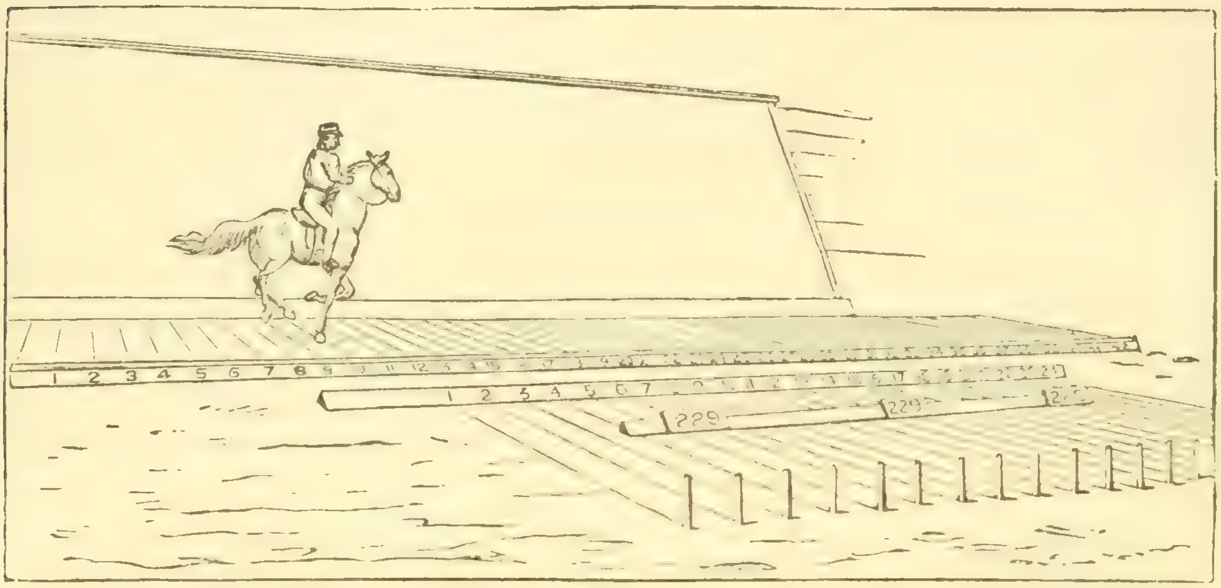
engraving), at a suitable height to strike the breast of the animal experimented with, one end of the thread being fastened to the background, the other to the spring, Fig. 4, which is drawn almost to the point of contact.

The animal in its progress over the track will strike these threads in succession, and as each pair of springs is brought into contact, the current of electricity thereby created effects a photographic exposure, as described by Figs. 2 and 4; and each consecutive exposure records the position of the animal at the instant the thread is struck and broken.

For obtaining successive exposures of horses driven in vehicles, one of the wheels is steered in a channel over wires slightly elevated from the ground; the depression of each wire completes an electric circuit, and effects the exposures in the same manner as the threads.

Fig. 6. Operating track, covered with corrugated indiarubber, and marked with transverse lines 12 inches apart. Each line is numbered, for the purpose of more readily ascertaining the length of the animal's stride. On one side of the track, and opposite to the battery of cameras, a white background is erected at a suitable angle.

FIG. 6.



The camera in which any one negative in a series of exposures is made is designated on that negative by the parallel direction of the vertical stake with the horizontal line extending to the corresponding number immediately opposite. The discriminating number of each series is marked on each negative by the large numbers—229, for example—which are changed for each movement illustrated.

For recording the successive attitudes of animals not under control, an apparatus is used, comprising a cylinder, around which are spirally arranged a number of pins; upon the cylinder being set in motion through gearing connected with a spring or weight, these pins are consecutively brought into contact with a corresponding number of metal springs; a succession of electric currents are thereby created which act through their respective magnets attached to the electro-exposors at regulated intervals of time. The cylinder is put in motion either by bringing it into gearing with other parts of the apparatus already in motion; or by releasing a break with the hand, or by the action of some object at a distance by means of an electric current.

This apparatus is principally used for illustrating the flight of birds, the motions of small animals, and changes of position without continuous progressive motion, such as occur during wrestling or turning a summersault; when the cameras are directed towards the place where the movements are being executed.

The boxes outside the studio (Fig. 5) contain cameras and electro-exposors for obtaining synchronous exposures of a moving object from different points of view.

The following analyses of some of the movements investigated by

the aid of electro-photographic exposures, are repeated by permission of the President and Council from a paper read by the author before the Royal Society, and are rendered more perfectly intelligible by the reproductions of the actual motions projected on a screen through the zoopraxiscope.

The Walk.

Selecting the horse for the purposes of illustration, we find that during his slowest progressive movement—the walk—he has always two, and, for a varying period, three feet on the ground at once. With a fast walking horse the time of support upon three feet is exceedingly brief; while during a very slow walk all four feet are occasionally on the ground at the same instant.

The successive order of what may be termed foot fallings are these. Commencing with the landing of the left hind foot, the next to strike the ground will be the left fore foot, followed in order by the right hind, and right fore foot. So far as the camera has revealed, these successive foot fallings during the walk are invariable, and are probably common to all quadrupeds. But the time during which each foot, in its relation to the other feet, remains on the ground, varies greatly with different species of animals, and even with the same animal under different conditions. During an ordinary walk, at the instant preceding the striking of the left hind foot, the body is supported on the right laterals, and the left fore foot is in act of passing to the front of the right fore foot. The two hind feet and the right fore foot immediately divide the weight. The right hind foot is now raised, and the left hind with its diagonal fore foot sustains the body; the left fore next touches the ground and for an instant the animal is again on three feet; the right fore foot is immediately raised and again the support is derived from laterals—the left instead of as before the right. One half of the stride is now completed, and a similar series of alternations, substituting the right feet for the left, completes the other half. These movements will perhaps be more readily understood by a reference to the longitudinal elevation, Fig. 7, No. 1, which illustrates some approximate relative positions of the feet of a rapid walking horse, with a stride of 5 feet 9 inches. The positions of the feet indicated in this, and also in the other strides illustrated in Fig. 7 are copied from photographs, and from them we learn that during an ordinary walk the consecutive supporting feet are:

1. The left hind and left fore—*laterals.*
2. Both hind, and left fore.
3. Right hind and left fore—*diagonals.*
4. Right hind and both fore.
5. Right hind and right fore—*laterals.*
6. Both hind, and right fore.
7. Left hind and right fore—*diagonals.*
8. Left hind and both fore.

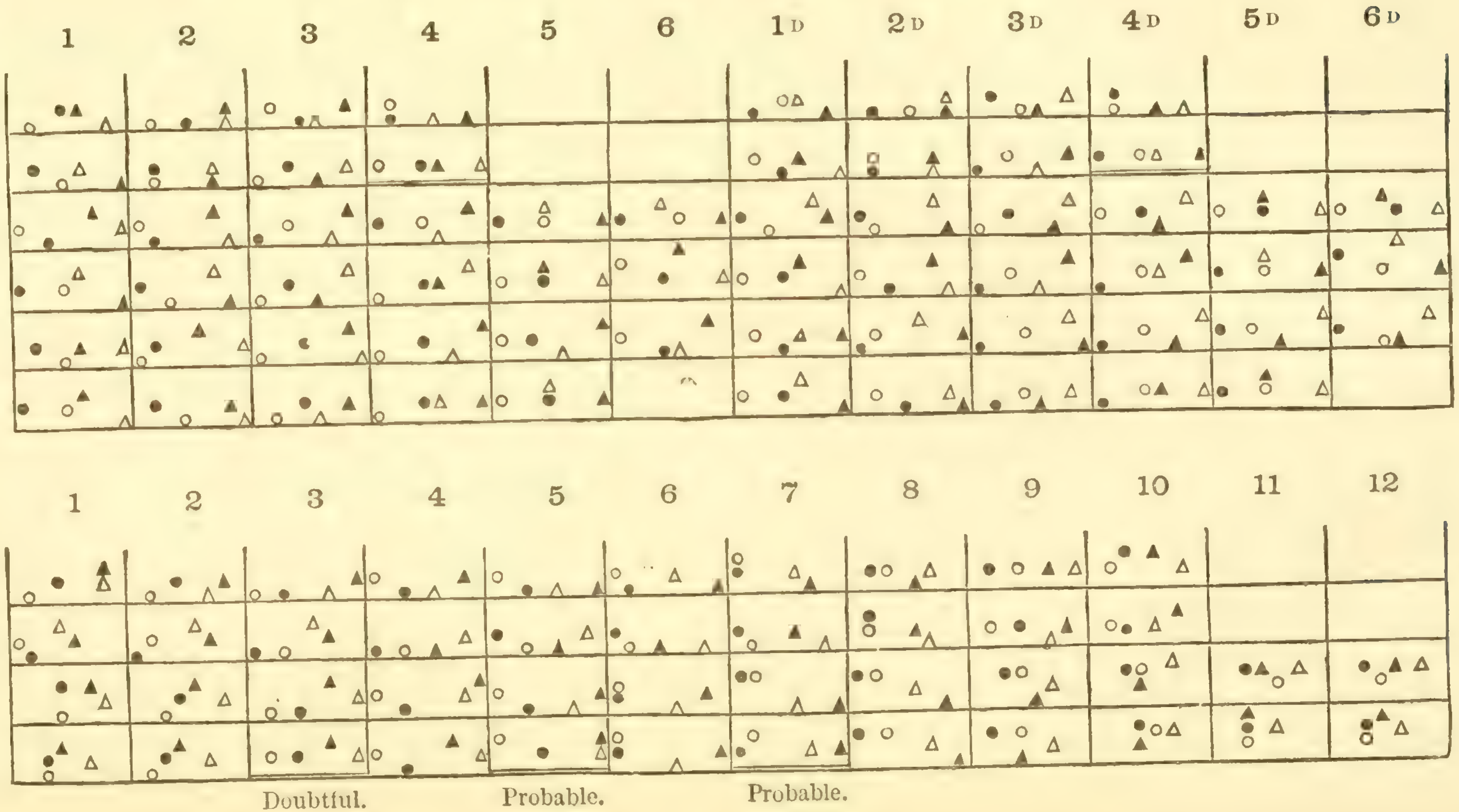
KEY.

	Rt.	*Left.*
Hind Feet ..	●	○
Fore Feet	▲	△
Line of ground		

FIG. 7.—LONGITUDINAL ELEVATION OF SOME CONSECUTIVE POSITIONS OF THE FEET OF HORSES DURING VARIOUS MOVEMENTS.

Each line illustrates a single complete stride. The comparative distances of the feet from each other, or from the ground, are approximate; not to scale. Direction of movement →

Action.	Length of Stride. ft.	in.
1. Walking ..	5	9
2. Trotting ..	7	6
3. " ..	17	6
4. " ..	18	3
5. Ambling ..	10	3
6. Racking ..	12	6
7. Cantering ..	10	3
8. Galloping ..	11	9
9. " ..	19	9
10. Galloping, a conjectural stride of 22 feet or more ..		



Commencing again with the first position; it is thus seen that when a horse during a walk is on two feet, and the other two feet are suspended between the supporting legs, the suspended feet are laterals. On the other hand, when the suspended feet are severally in advance of and behind the supporting legs, they are diagonals.

These invariable rules seem to be neglected or entirely ignored by many of the most eminent animal painters of modern times.

The Trot.

By some observers the perfect trot is described as an absolutely synchronous movement of the diagonal feet. This simultaneous action may be considered desirable, but it probably never occurs.

Sometimes the fore foot will be raised before the diagonal hind foot, sometimes afterwards; but in either instance, the foot raised first will strike the ground first; repeated experiments with many racing and other trotting horses confirmed this want of simultaneity. Selecting for an example of the trot a horse making a stride of 18 feet in length, we find that at the instant his right fore foot strikes the ground, the left hind foot is a few inches behind the point where it will presently strike at about 38 or 40 inches to the rear of the fore foot. When both feet have reached the ground, the right hind leg is stretched back almost to its fullest extent, with the pastern nearly horizontal, while the left fore leg is flexed under the body. As the legs approach a vertical position the pasterns are gradually lowered, and act as springs to break the force of the concussion until they are bent nearly at right angles with the legs.

At this period the left fore foot is raised to its greatest height, and will frequently strike the elbow, while the right hind foot is but little raised from the ground and is about to pass to the front of the left hind.

The pasterns gradually rise as the legs decline backwards until the right fore foot has left the ground and the last propelling force is being exercised by the left hind foot; which accomplished, the animal is in mid air.

The right hind foot continues its onward motion until it is sometimes much in advance of its lateral fore foot, the former, however, being gradually lowered, while the latter is being raised. The right hind and both fore legs are now much flexed, while the left hind is stretched backwards to its greatest extent with the bottom of the foot turned upwards, the left fore leg is being thrust forwards and gradually straightened, with the toe raised as the foot approaches the ground; which accomplished, with a substitution of the left limbs for the right we find them in the same relative positions as when we commenced our examination, and one half of the stride is completed.

With slight and immaterial differences, such as might be caused by irregularities of the ground, these movements are repeated by the other pair of diagonals, and the entire stride is then complete.

Line 4 illustrates a stride of 18 feet 3 inches, and the order of supporting feet are:—

1. The right fore foot.
2. The left hind and right fore feet.
3. The left hind foot.
4. Without support.
5. The left fore foot.
6. The right hind and left fore feet.
7. The right hind foot.
8. Without support.

It appears somewhat remarkable that until the results of M. Marey's experiments and of those obtained by electro-photography were published, many experienced horsemen were of opinion that during the action of trotting at least one foot of a horse was always in contact with the ground.

If the entire stride of a trotting horse is divided into two portions, representing the comparative distances traversed by the aggregate of the body while the feet are in contact with, and while they are entirely clear of, the ground; the relative measurements will be found to vary very greatly, they being contingent upon length of limb, weight, speed, and other circumstances.

Heavily built horses will sometimes merely drag the feet just above the surface, but, in every instance of a trot, the *weight* of the body is really unsupported twice during each stride (see stride 2, positions 4 and 4 D). It sometimes happens that a fast trotter, during the two actions of a stride, will have all his feet clear of the ground for a distance exceeding one-half of the length of the entire stride; this elasticity of movement is however exceptional.

The action of a fast-trotting horse while drawing a vehicle is very different from his action under the saddle; in the latter case, the hind legs are kept thrust back for a longer period, and their final forward movement is much more rapid.

The Amble.

Assuming our observation of this movement to commence when, during a stride of about 10 feet, the left hind foot has just struck the ground slightly to the rear of where the right fore foot is resting; the left fore leg will be well advanced but still flexed, with the toe pointed downwards, and the right hind foot having been the last to leave the ground, will be thrust backwards with the pastern nearly horizontal.

As the right fore foot leaves the ground, the left fore leg is gradually straightened during its thrust forwards; the right hind foot in the meantime is gradually advancing, and the horse is supported on the left hind foot alone.

The left fore foot is now brought to the ground, and the body rests on the left laterals, with the right laterals suspended between them.

As the left fore leg attains a vertical position, its lateral leaves the ground, and the support of the body devolves on the left fore foot alone, the right fore leg being considerably flexed, with the foot in advance of the left fore leg.

The right hind foot now strikes the ground, and one half of the stride is accomplished; these movements are repeated with a change of the limbs for the remaining portion of the stride, and the horse is again in the position in which we first observed him.

We shall see by reference to stride No. 5 the consecutive supporting feet to be:

1. The left hind foot.
2. The left hind and left fore feet—*laterals.*
3. The left fore foot.
4. The left fore and right hind feet—*diagonals.*
5. The right hind foot.
6. The right hind and right fore feet—*laterals.*
7. The right fore foot.
8. The right fore and left hind feet—*diagonals.*

The right fore foot being raised, the horse is again in the first position.

The amble and the walk are the only regular progressive movements of the horse wherein the body is never without the support of one or more legs, in all others the weight is entirely off the ground for a longer or shorter period.

The Rack or Pace.

The rack differs from the trot in the nearly synchronous action of the *laterals* instead of the *diagonals.*

In some countries the rack is naturally adopted by the horse as one of his gaits, but it is probably caused by the effects of training exercised over many generations of his ancestors.

The movements already described are regular in their action, and a stride may be divided into two parts, which are essentially similar to each other.

The Canter

and the gallop, however, cannot be so divided, and a complete stride in either of those gaits is a combination of several different movements.

The canter is usually regarded as a slow gallop, probably from the facility with which a change from one gait to the other can be effected; an important difference will, however, be observed.

Assuming a horse after his propulsion through the air, during a stride of 10 feet, to have just landed on his left hind foot, the right hind foot will be on the point of passing to the front of the left. The left fore leg will be thrust forward and nearly straight, while the right fore leg will be flexed with the foot elevated about 12 inches from the ground, and somewhat behind the vertical of the breast.

The left fore foot being brought to the ground, the body is supported by the laterals; the right hind foot is, however, quickly lowered, and performs its share of support. The left hind foot is then raised, and the right hind and left fore legs assume the weight, the former being nearly vertical, and the latter inclined well back, the right fore foot is thrust well forward, and is just about to strike the ground; when it does, three feet again share the support, they being the two fore and the right hind. The left fore foot now leaves the ground, and we again find the support furnished by the laterals, the right instead of, as before, the left.

The right hind foot is raised when the right fore leg becomes vertical; this latter, which now sustains the entire weight, gives the final effort of propulsion, and the body is hurled into the air.

The descent of the left hind foot completes the stride, and the consecutive movements are repeated.

In stride No. 7 we learn that during the canter the support of the body is derived from

1. The left hind foot.
2. The left hind and left fore feet—*laterals.*
3. Both hind and the left fore feet.
4. The right hind and left fore feet—*diagonals.*
5. The right hind and both fore feet.
6. The right hind and right fore feet—*laterals.*
7. The right fore foot alone, on which he leaves the ground.

The Gallop or Run.

This movement has in all ages been employed by artists to convey the impression of rapid motion, although, curiously enough, the attitude in which the horse has been almost invariably depicted is one which is impracticable during uniform progressive motion.

When during a rapid gallop, with a stride of 20 feet, a horse after his flight through the air lands on his left hind foot, the right hind will be suspended over it at an elevation of 12 or 15 inches, and several inches to the rear of and above it the sole of the right fore foot will be turned up almost horizontally, the left fore leg is flexed with the foot under the breast at a height of 18 or 20 inches.

The right hind foot strikes the ground some 36 inches in advance of the left hind, each as they land being forward of the centre of gravity.

The body is now thrust forward, and while the right hind pastern is still almost horizontal, the left hind foot leaves the ground. At this time the left fore leg is perfectly straight, the foot, with the toe much higher than the heel, is thrust forward to a point almost vertical with the nose, and at an elevation of about 12 inches the right fore knee is bent at right angles, and the foot suspended under the breast at several inches greater elevation than the left fore foot.

The left fore foot now strikes the ground, 96 inches in advance of the spot which the right hind foot is on the point of leaving, and for

a brief space of time the diagonals are upon the ground together. The left fore leg, however, immediately assumes the entire responsibility of the weight, and soon attains a vertical position, with its pastern at right angles to it.

In this position the right hind foot is thrust back to its fullest extent, at an elevation of 12 or 14 inches, with the pastern nearly horizontal. The left hind foot is considerably higher and somewhat more forward; the right fore leg is straight, stretched forward, with the foot about 15 inches from the ground, and almost on a perpendicular line from the nose. The right fore foot strikes the ground 48 inches in advance of the left fore, which, having nearly performed its office, is preparing to leave the ground; the animal will then be supported on the right fore foot alone, which immediately falls well to the rear of the centre of gravity, which is sometimes passed by the left hind foot at a height of about 12 inches; the right hind foot is some distance in the rear, and the left fore foot, at a height of 24 inches, is suspended somewhat in advance of its lateral.

In this position the horse uses the right fore foot for a final act of propulsion, and is carried in mid air for a distance of 60 inches, after which the left hind foot descends, the stride is completed, and the consecutive motions renewed.

The measurements and positions herein given do not pretend to exactness, as they must depend to some extent upon the capability, training, and convenience of the animal; but they may be accepted as representing an average stride of 20 feet with a horse in a fair condition for racing.

From this analysis it will be seen, by reference to stride 9, that a horse, during an ordinary gallop, is supported consecutively by:

1. The left hind foot,
2. Both hind feet,
3. The right hind foot,
4. The right hind and left fore feet,
5. The left fore foot,
6. Both fore feet,
7. The right fore foot,

with which he leaves the ground, while the only position in which we find him entirely without support is when all the legs are flexed under his body.

It is highly probable, however, that more exhaustive experiments with long-striding horses in perfect training, will discover there is sometimes an interval of suspension between the lifting of one fore foot and the descent of the other; and also between the lifting of the second hind foot which touches the ground, and the descent of its diagonal fore foot (see imaginary stride 10). Should this latter be the case, it will, from the necessary positions of the other limbs, afford but a very shadowy pretext for the conventional attitude used by artists to represent a gallop. It is extremely doubtful if there can be any interval of suspension between the

lifting of one hind foot and the descent of the other, no matter what the length of stride.

Many able scientists have written on the theory of the gallop, but I believe Marey was the first to demonstrate, that in executing this movement, the horse left the ground with a fore foot and landed on a hind foot.

The Leap.

There is little essential difference in general characteristics of either of the several movements that have been described, but with a number of experiments made with horses while leaping, no two were found to agree in the manner of execution. The leap of the same horse at the same rate of speed, with the same rider, over the same hurdle, disclosed much variation in the rise, clearance, and descent of the animal. Apart from this, the horses were not thoroughly trained leapers, and the results are perhaps not representative of those that would be obtained from the action of a well-trained hunting horse. A few motions were, however, invariable. While the horse was raising his body to clear the hurdle, one hind foot was always in advance of the other, and exercised its last energy alone.

On the descent, the concussion was always received by one fore foot, supported by the other more or less rapidly, and sometimes as much as 30 inches in advance of where the first one struck, followed by the hind feet, also with intervals of time and distance between their several falls. It is highly probable future experiments will prove these observations to be invariable in leaping.

It is highly probable that these photographic investigations, which were executed with wet collodion plates with exposures not exceeding in some instances the one five-thousandth part of a second, will dispel many popular illusions as to gait, and that future and more exhaustive experiments, with all the advantages of recent chemical discoveries, will completely unveil to the artist all the visible muscular action of men and animals during their most rapid movements.

The employment of automatic apparatus for the purpose of obtaining a regulated succession of photographic exposures is too recent for its value to be properly understood, or to be generally used for scientific experiment; at a future time, the pathologist, the anatomist, and other explorers for hidden truths will find it indispensable for their complex investigations.

[E. M.]

WEEKLY EVENING MEETING.

Friday, March 17, 1882.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Captain W. de W. Abney, R.E. F.R.S.

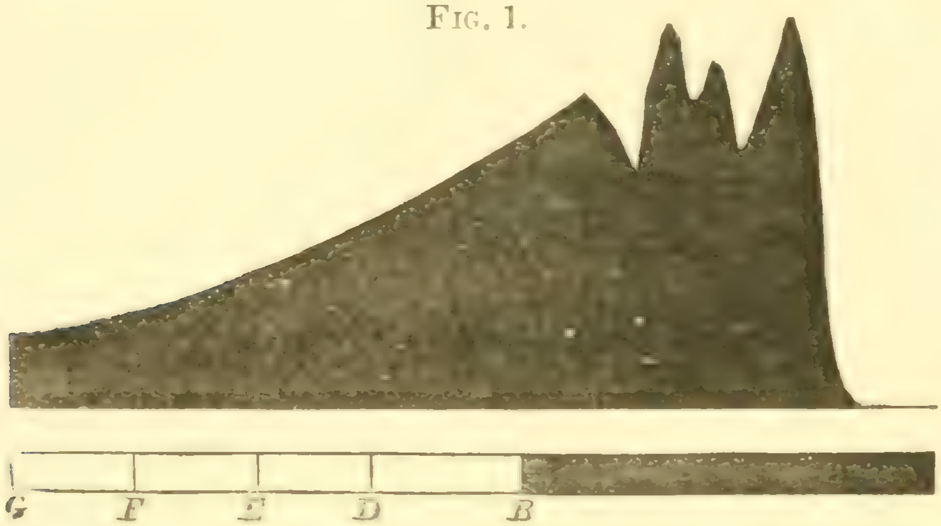
Spectrum Analysis in the infra red of the Spectrum.

At the Royal Institution it would be almost an impertinence on my part were I to attempt to prove the existence of dark rays which lie below the red of the spectrum: Professor Tyndall has made you all so thoroughly acquainted with these dark rays that I may assume not only your acquaintance with them, but also your interest in them. The most accessible means hitherto of exploring this region of the spectrum has been by a study of its heating effect, as shown by the thermopile; and more particularly when an integration of the effect produced by its component rays is required. Beautifully delicate is the thermopile, but it must not be forgotten that it depends for its delicacy largely on the area of its face. A little consideration will show that if you wish to examine the spectrum by its means, you must be prepared for a drawback.

In the thermopile I have here, the elements composing it are arranged in a line, and I can allow any fineness of beam to penetrate to its surface by means of this adjustable slit. Unfortunately, however, the narrower the slit the smaller is the heating effect on the thermopile; so that, if I wish to measure the heating effect of a very narrow beam of radiation which may find its way between the jaws of the slit, the heating effect will be so small that it may escape detection. In the visible solar spectrum, as you are all aware, there are breaks of continuity presenting to the eye the appearance of fine dark lines, and showing a diminished radiation. Now, suppose for an instant that the part of the spectrum which is visible to us was dark, not exciting vision, it is manifest, even were the energy of this part of the spectrum very much greater than it is, that to ascertain the existence of these fine lines by means of the thermopile would be next to impossible; since the slit would have to be closed to an excessive degree of fineness, and when so closed the thermopile would be insensible to radiations when such existed. Now this is precisely the case with which we have to deal in the spectrum below the red. We know of its existence; but with any source of radiation, such as the sun for instance, the thermopile would stand but a small chance of finding any narrow breaks in its continuity. I need

only refer to Lamansky's thermogram (Fig. 1) of the solar spectrum, taken by a linear thermopile, in which, after taking extraordinary precautions, he found the existence of three breaks. The thermopile, with its galvanometer, takes no cognizance of time; given a constant and steady flux of radiation striking it during a certain time, the electrical current generated, which is shown by the deflection of the galvanometer needle, remains constant, and no increase of time gives a greater deflection. If we could imagine a thermopile the current generated by which, by the efflux of time, would proportionally increase the deflection of the needle, then the most limited radiation

Fig. 1.



striking the pile could be measured and calculated. At present there seems to be no method capable of taking account of time for this purpose except photography, and until recently it seemed chimerical to apply it. I should like to show you how photography takes cognizance of time, and also why it seemed unsuitable for investigating the dark rays below the red end of the spectrum. I propose to photograph the spectrum on an ordinary photographic compound.

A piece of paper has been coated with silver bromide, and this compound, when placed in the spectrum, is acted upon by the ultra violet, the violet and the blue rays. If I cover up $\frac{2}{3}$ of the slit of the lantern and allow an exposure of the paper to the spectrum of two seconds, then with the next $\frac{1}{3}$ of the slit an exposure of ten seconds, and with the remaining $\frac{1}{3}$ give an exposure of thirty seconds, it will be seen when I develop the image that the length of the spectrum varies, and also the darkening of the different portions. Thus the longest exposure will show the greatest intensity of action and the greatest length of spectrum. [Shown.] This experiment serves a double purpose, for whilst it makes it clear that photography takes cognizance of time, yet it seemingly shows that it is unfitted for exploring the infra-red region, since ordinary photographic compounds are unacted upon by them. Could a compound be found which was sensitive to the dark rays, it is manifest that the battle would be won, and that investigations full of interest might be the outcome.

Some eight years ago I tried my hand at the matter, and after several years of experimenting it was my good fortune to find a compound which was chemically acted upon by the dark radiations.

I will not weary you with the various experiments undertaken; suffice it to say that silver bromide was selected as the salt to work upon. My aim was to prepare an emulsion of bromide of silver in collodion (an emulsion being silver bromide in a fine state of division suspended in collodion) which should transmit green-blue light. Let me show you why.

The spectrum on the screen, when unabsorbed by any medium, shows every colour. If, however, a piece of green glass is inserted before the slit, it is seen at once that the violet is absorbed and also the lowest part of the red. Inferentially it may be supposed that the infra-red rays are also absorbed. Orange is the usual colour of the silver bromide, and a piece of orange glass placed in front of the slit cuts off from the spectrum all the most refrangible part of the spectrum and none of the least refrangible.

On the principle of conservation of energy, where radiation is absorbed, there work must be done by the absorbing body and show itself as heat or chemical action. Heat with the thermopile, for instance, and chemical action with the salt of silver. Thus, with the orange bromide we should expect, as we have already seen is the case, that the violet and blue rays would do work on it whilst the other rays would be passive.

The green state was attained after much labour. The colour of this new preparation of silver bromide, and that of the old, are now shown by means of the lantern on the screen.

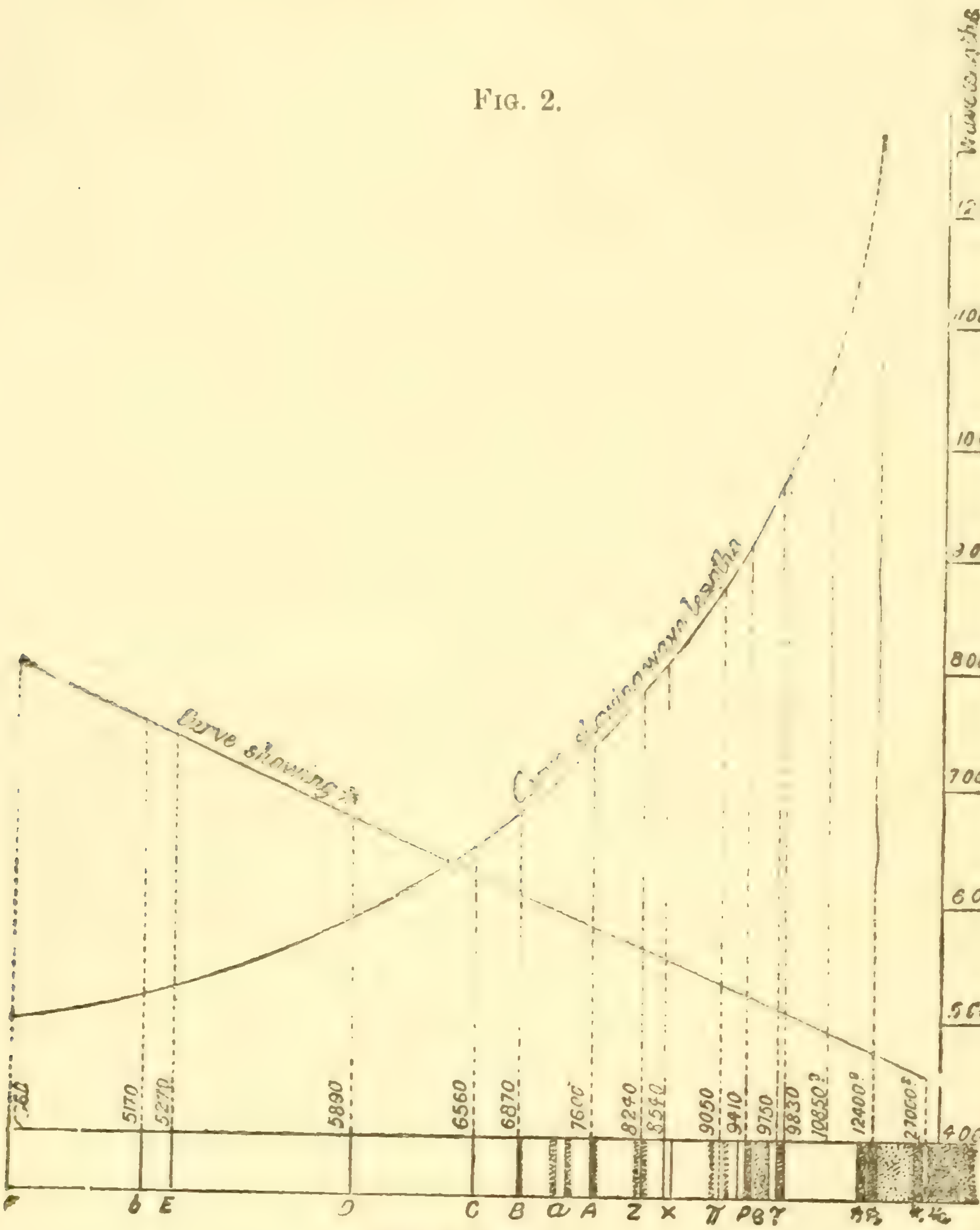
Now you will see that if the work done in the green bromide was chemical decomposition, the problem was solved, and that the unknown might be made to write down, in hieroglyphics perhaps, but still in a manner capable of being deciphered, its character and peculiarities.

I will endeavour to experimentally illustrate that the green compound *is* acted upon by the dark rays. It will be in your recollection that Professor Graham Bell's recently introduced photophone is in reality an instrument consisting of a perforated disc rotating in front of a source of radiation, and by means of a lens the radiant energy is focussed on the surface of a selenium cell, to which a telephone is attached, and that by this means a musical sound is produced in the telephone.

Professor Bell showed that the same effect was produced when a piece of ebonite was introduced between the source of light and the selenium cell. Dr. Huggins proposed to me that I should try the permeability of the ebonite by the dark rays; and this was done, with the result that the spectrum was taken through it, showing an impression on the green bromide of the dark rays. An image of the incandescent carbon points of the electric light are now formed on a piece of ebonite, and behind it is a glass plate covered with the

bromide; an exposure of twenty seconds will suffice to impress the image of the points by their dark rays. [The image was developed and subsequently shown.] It will be seen that the bromide in this state is somewhat sluggish to respond to the vibrations of the dark rays. I will now make an experiment to show how different is the behaviour of the orange bromide. Behind this rotating disc,

FIG. 2.



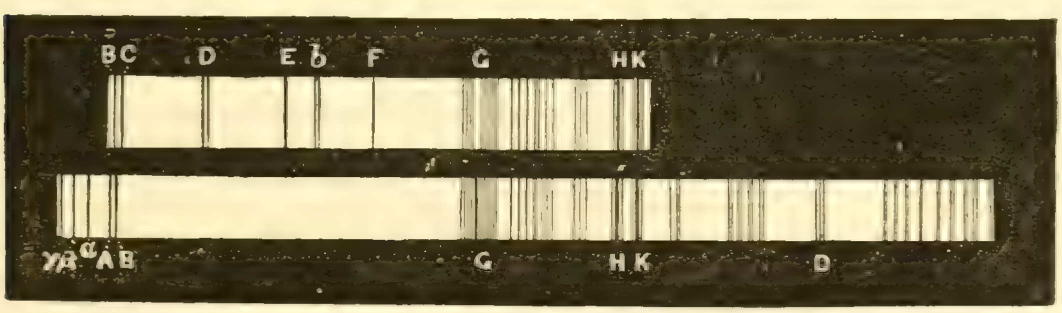
Prismatic Solar Spectrum.
Measured from a Photograph.

which is made up of alternate transparent and opaque sectors, is a plate prepared with the orange bromide. A spark $\frac{1}{5}$ of an inch in length from a battery of Leyden jars is sufficient to impress a sharp image of the sectors on the plate though they are rapidly rotating. [The exposure was made to the spark whilst the disc

was rotating; and developed before the audience and subsequently the photograph was shown.] The exposure is estimated by Cazin as $\frac{47}{1000000}$ of a second. It would require twenty such sparks to impress the red end of the spectrum on a pure bromide plate. I should wish to show you one more remarkable example of the action of the dark rays on the blue-green silver bromide. On the screen we have a slide showing certain opaque discs and triangles. They were produced in the following manner: A card was perforated with such discs and triangles, and placed $\frac{1}{8}$ of an inch above a green bromide plate; above this was suspended a kettle of boiling water, and the radiation from the kettle acted on the plate through these holes, with the result that, after considerable exposure, an image of the holes was developed on the bromide below. This shows that this particular form of silver salt responds to waves of very low refrangibility.

The first application of the new compound was to the solar spectrum, and on the screen we have the first impression of the infra-red region ever taken. On the diagram (Fig. 2) there are bands ϕ and ψ drawn which do not appear in the photograph; only on two occasions have they been impressed, for reasons which will be explained. To show you how far our knowledge of this region is extended, a photograph is shown on the screen of the spectrum obtained photographically by Draper, which he obtained by indirect means.

FIG. 3.



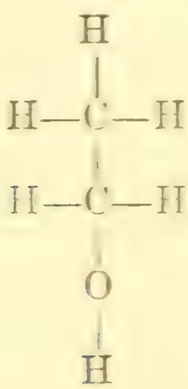
When a grating of large dispersion replaces the prism, the bands are broken up into lines; and very beautiful lines they are in some cases. From such photographs a wave-length map was made. [Shown]. The line of greatest wave-length impressed in the spectrum is 22,000. Now the visible part of the spectrum extends from λ 3800 to λ 7600; thus the invisible spectrum, as photographed, is *five times* longer than the visible spectrum.

In the visible portion of the solar spectrum, most of the lines have been traced to the absorption of different metallic or other vapours existent in the solar or our own atmosphere. The cause of the absorptions in the invisible part of the spectrum are as yet untraced, except in one or two instances to which I shall have to allude presently. With the exception of sodium and calcium, no metallic vapours seem to have what would be bright lines, were they visible, in the infra-red portion of the spectrum; hence we are

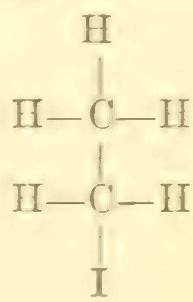
almost bound to suppose that absorption lines in this region are really due to compound bodies of some description. Knowing the results that Professor Tyndall had got with the hydrocarbon and other vapours and liquids in the infra-red region by thermopile integration, Colonel Festing and myself determined to see if we could disintegrate Professor Tyndall's integrations, and locate in the invisible spectrum the absorptions which he had noted.

We commenced with water, and were delighted to find that water gave a very definite spectrum; and I propose to show the method adopted for this research. In front of the slit of the spectroscope, which has three prisms, was placed a tube of water or other liquid, in some cases of the length of two feet, but more generally of six inches. The crater or bright luminous patch from the positive pole of the electric light was projected on the slit, the rays having to traverse the liquid. The image of the spectrum was then received on a sensitive plate. In this manner I propose to take the spectrum of a two-foot length of water. [The photograph was taken and subsequently shown on the screen.] Before proceeding further, I will again show you the superior sensitiveness of the orange form of bromide for blue rays over the green bromide for the dark rays. The same length of spark as before shall be used, and the light from it projected by means of a lens through a couple of prisms, and the image be focussed on an orange bromide film. The spark passes, and the most refrangible end of the spectrum will be found to be impressed. [The photograph was developed before the audience, and at the close of the lecture thrown upon the screen.] Our next attempts were with alcohol and ether, and in these we got many definite absorptions, some lined and some banded, the bands being more or less shaded. On trying ethyl iodide, however, we came upon a spectrum which was composed of fine lines and bands with comparatively sharp edges, differing in this respect from the two former spectra. The difference in composition between alcohol and ethyl iodide is shown in the following diagram.

Alcohol.



Ethyl iodide.



The prime difference is the presence of oxygen in the one and its absence in the other. We next tried methyl iodide, and got a simpler form of spectrum than the ethyl iodide. It now struck us

that if we could diminish the hydrogen we might have something again different.

Methyl iodide.	Chloroform.	Carbon tetrachloride.
H	H	Cl
|	|	|
H—C—H	Cl—C—Cl	Cl—C—Cl
|	|	|
I	Cl	Cl

We therefore tried chloroform, and much to our surprise all the broad bands had vanished, and we had a linear spectrum. What would happen if we removed the last hydrogen? In carbon tetrachloride the last hydrogen is removed, and we got no absorption spectrum at all; indicating that hydrogen had a most important bearing on the absorptions. Carbon disulphide and cyanogen gave us the same results. On the other hand, when we spectroscoped hydrochloric acid we had a linear spectrum, as we had with ammonia, sulphuric acid, and nitric acid. Even water was found not to be free from lines, as the boundary of each band was a line. I think, then, that this satisfactorily settles the point that hydrogen gives the initiative to all the special absorptions we noticed. The introduction of oxygen gives shaded bands; but on measurement it was found that shades were made up of step by step absorptions between two or more positions of hydrogen lines. Again, what is called the radical of each group of compounds was found to have a definite absorption in a definite locality; hence this spectroscopic method became a means of qualitatively determining the composition of an unknown compound and its molecular structure. I would point out also how the absorptions found by the photographic method go hand in hand with those found by Professor Tyndall. In the annexed table we have the value of the absorptions found by him, and following the absorption spectra of the same bodies through six inches of liquid. The coincidence is remarkable and worthy of attention.

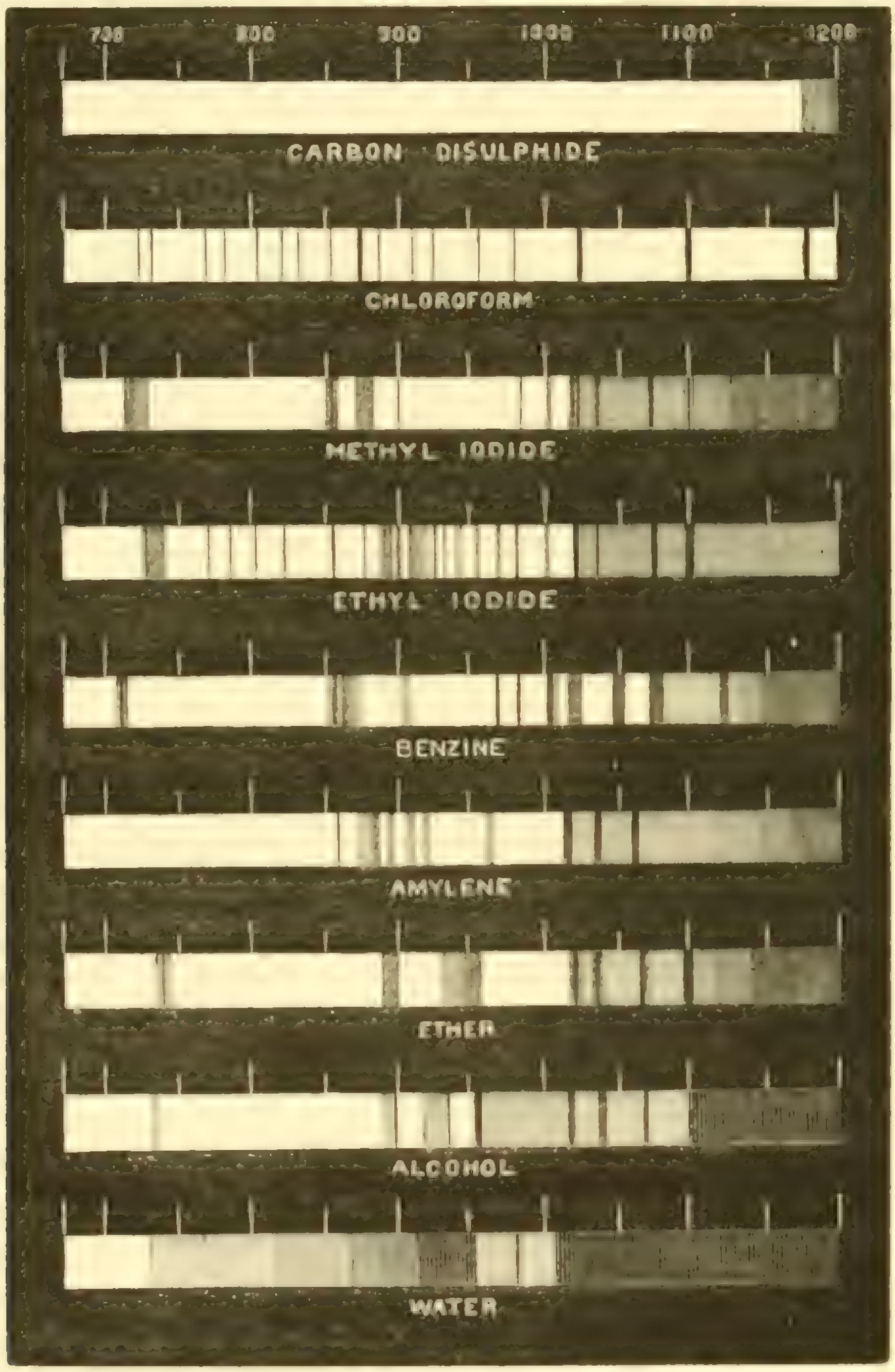
ABSORPTION OF HEAT BY LIQUIDS.

(*Source of Heat a Platinum Spiral raised to Bright Redness by a Voltaic Current.*)

Liquid.	Thickness in Parts of an Inch.	
	0·02.	0·27.
Carbon disulphide	5·5	17·3
Chloroform	16·6	44·8
Methyl iodide	36·1	68·6
Ethyl iodide	38·2	71·5
Benzine	43·4	73·6
Amylene	58·3	82·3
Ether	63·3	85·2
Alcohol	67·3	89·1
Water	80·7	91·0

Your attention should be drawn more especially to the water spectrum, in which it will be seen what a large proportion of the infra red is cut off by even a small thickness. Aqueous vapour absorbs in the same locality as the water; hence you will see

Fig. 4.



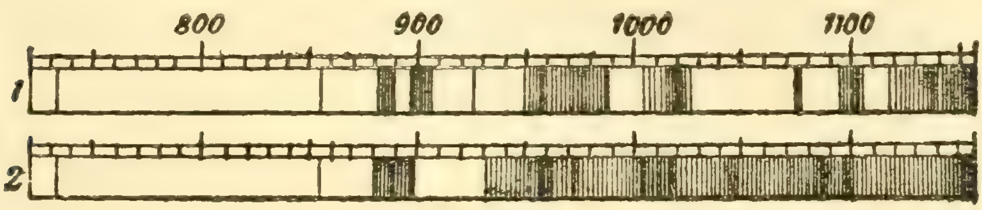
how it is that I have photographed the bands ϕ and ψ in the solar spectrum but seldom (see Fig. 2). When they were impressed a very dry and biting north-east wind was blowing, which enabled this part of the spectrum to find its way through our atmosphere.

On comparing the solar spectrum with the absorption spectra of the above organic bodies, we found that the principal lines of the benzine and ethyl series found a place in the solar absorptions; and for reasons which I have not time to enter into now, we were induced to locate these bodies outside our atmosphere. In what part of space

they exist is a moot point, but there is no doubt that they are somewhere present in it. That alcohol is to be found in the sun would be perhaps to stretch a point too far; and it would be unwise to wish to find it there, as it might rouse the animosity of a small section of the community against this branch of spectrum analysis. The ingredients to make it are there, however, without any doubt.

In regard to these same class of spectra it is interesting to find that there is a marked difference in bodies containing the same relative proportions of carbon, hydrogen, and oxygen, but molecularly different. Take for example aldehyde and paraldehyde (Fig. 5). A

Fig. 5.



No. 1 Aldehyde
No. 2 Paraldehyde

molecule of the latter contains three molecules of the former, and we see that their spectra differ materially. If such be the case in organic compounds, we may surely expect to find the same difference in the spectra of the elements, if there is a different molecular grouping of them at different temperatures. Whether the changes in metallic spectra are due to this cause has still to be proved, though it seems probable that it may be so.

The latest point in our research in this subject which will be of interest to chemists appears to be the possibility of distinguishing between para- and ortho-organic compounds. Our experiments so far demonstrate that this can be done. If on further research it prove to be the case, this branch of spectrum analysis would be worthy of study by chemists for this reason alone.

[W. de W. A.]

WEEKLY EVENING MEETING,

Friday, March 24, 1882.

Warren De La Rue, Esq. M.A. D.C.L. F.R.S. Vice-President, in the Chair.

Professor W. E. Ayrton, F.R.S.

Electric Railways.

We have grown so accustomed to the regular announcement—"serious accident on such and such a railway, several passengers injured"—that we have almost come to regard railway accidents as inevitable, just as parents mistakingly think the measles and whooping cough necessary accompaniments of childhood. But speed no more means disaster than a densely crowded city means disease. The first effect of overcrowding is undoubtedly to produce fever and other complaints. If, however, the knowledge and practice of the laws of hygiene increase more rapidly than the population of a town, the death rate, as we have seen, diminishes, instead of augmenting. And so it is with locomotion; the stage-coach journeys of our ancestors were slow enough for the most staunch conservative, and yet the percentage of the passengers injured on their journeys was far greater than even now with our harum-scarum railway travelling. The number of passengers has increased enormously, but the safety has increased in an even greater rate. If then we can devise methods introducing still greater security, a far larger number of passengers may travel at a far greater speed and with less fear of danger than at present.

Accidents constitute one charge against railway conveyance, but there is another, and that is the cost. Cheap as railway travelling now is, compared with the departed stage-coach locomotion, the price of the tickets is still far too high for railways to fulfil, even in a small degree, one of their most important functions, and that is transporting labourers from parts of the country where labour is scarce, to others where it is abundant and labourers in demand.

But how is a happier state of things to be realised? We cannot expect the railway companies to lower their fares merely to benefit humanity. If, however, we can prove to them that the present system of railways is neither the most remunerative to themselves nor the most beneficial to the community at large, we may hope to win the attention of railway directors, whose stock question is, and quite rightly, "Will it pay?"

Those of you who have read the life of Stephenson know what a protracted fight he had to carry one of his most cherished ideas, and that was the employment of a locomotive engine to draw the train, instead of a stationary engine to pull it with ropes or chains. His

adversaries saw the disadvantage of adding the weight of the locomotive to the weight of the train, whereas Stephenson was especially struck with the enormous waste of power in the friction of ropes or chains passing over pulleys. [Experiments were then shown proving, *first*, that the mass of the locomotive necessitated the engine having a greater horse-power to get up the speed of the train quickly as well as a greater horse-power to keep up the speed; *secondly*, that the friction and wear and tear of ropes, such as were employed on the London and Blackwall Railway, would have been an insuperable hindrance to the development of railways.] From this was deduced that, since in Stephenson's day the only feasible mode of communicating the power of a stationary engine to a moving train was by means of ropes, his decision to adopt the locomotive was perfectly correct at the time it was made.

Attempts have been made to propel trains by blowing them through tubes, or by blowing a piston attached to the train through a tube, but such attempts at pneumatic railways have nearly all been abandoned. The employment of air compressed into a receiver on the train by fixed pumping engines stationed at various points along the line, and employed to work compressed air engines on the carriages has been effected with considerable success by Colonel Beaumont, especially for tram-lines. The weight of the compressed air engine is, however, still very considerable. Any system of pumping water through a pipe and employing the water to work a hydraulic engine on the train is hardly worth considering, seeing that the mechanical difficulties of keeping up a continuous connection between the moving train and the main through which the water is pumped seems insuperable. Gas-engines worked with ordinary coal gas, stored perhaps under pressure, might be employed on the moving train, but the advantage arising from the absence of boiler and coal would be more than compensated for by the fact, that the weight of a gas-engine per horse-power developed is so much greater than that of a steam-engine. None of these systems, then, of dispensing with a locomotive is by any means perfect, and the success of the recent experiments on the electric transmission of power has turned the attention of engineers to the consideration, whether electricity could not successfully supplant steam for the propulsion of trains and tram-cars; whether it could not, in fact, supply an efficient means of transmitting power, the absence of which caused Stephenson to abandon ropes in favour of a heavy locomotive engine.

The whole question, like every similar one, is mainly a question of expense; and what we have to consider is, whether electric transmission on the whole leads to greater economy than can possibly be obtained by the employment of any kind of locomotive. The average weight of a locomotive is about that of six carriages full of people; ten carriages compose an ordinary train, hence the presence of the mass of the locomotive adds at least 50 per cent. to the horse-power absolutely necessary to propel the carriages alone, and therefore at

least 50 per cent. to the amount of coal burned. But there is another most serious objection to the engines, perhaps even more important than the preceding. The heavy engine passing over every part of the line necessitates the whole line and all the bridges being made many times as strong, and therefore many times as costly, and the expense of maintenance consequently also far greater, than if there were no locomotive. And it is not possible to make the engine much lighter; for it would not have then sufficient adhesion with the rails to be able to draw the train; in fact, you cannot diminish the weight as long as the train is propelled with only one or two pair of driving wheels as at present. The employment of electricity, however, will enable a train to be driven with every pair of wheels, just as the employment of compressed air enables every pair of wheels to brake the train.

To propel a train we must either utilise the energy of coal by burning it, or use the energy possessed by a mountain stream, or the energy stored up in chemicals, and which is given out when the chemicals are allowed to combine, or we must employ the energy of the wind. Practically we employ at present only the first store for propelling railway trains—the potential energy of coal; and that is to a great extent the store on which we shall still draw, even when we employ Electric Railways. For experience shows that, with the modern steam-engine and dynamo, at least one-twentieth of the energy in coal can be converted into electric energy; and that this is at least twenty times as economical as the direct conversion of the energy of zinc into electric energy by burning it in a galvanic battery.

But it may be asked, did not Faraday's discovery, in 1831, that a current could be produced by the relative motion of a magnet and a coil of wire, settle this point half a century ago? Theoretically—yes; practically, however, the problem was very far from being solved, because the dynamo machine was very unsatisfactory, and it was not until Pacinotti, in 1860, suggested the solution of the problem of obtaining a practically continuous current from a number of intermittent currents, and until Gramme, about 1870, carried out Pacinotti's suggestion in the actual construction of large working machines, that the mechanical production of currents became commercially possible. [Experiments were then shown illustrating the complete electric transmission of power, a gas-engine on the platform giving rapid motion to a magneto-electric machine, and the current thereby produced sent through an electro-motor at the other end of the room, which worked an ordinary lathe.]

In electric transmission of power there is not only waste of power from mechanical friction, but also from electric friction arising from the electric current heating the wire, through which it passed.

It was then explained and demonstrated experimentally that this latter waste could be made extremely small by placing so light a load on the electro-motor, that it ran nearly as fast as the generator or dynamo, which converted the mechanical energy into electric energy; actual experiments leading to the result that for every foot-pound of

work done by the steam-engine on the generator, quite $\frac{7}{10}$ of a foot-pound of work can be done by the distant motor.

One reason why electric transmission of power can be effected with so little waste is because electricity has apparently no mass, and consequently no inertia; there is, therefore, no waste of power in making it go round a corner, as there is with water or with any kind of material fluid. Another reason why electro-motors are so valuable for travelling machinery is on account of the light weight of the motor. Experiment shows, that one horse-power can be developed per 50 lbs. of dead weight of electro-motor; a result immensely more favourable than can be obtained with steam, gas, or compressed-air engines.

In addition to the loss of power arising from the heating of the wires by the passage of the current, there is another kind of loss that may be most serious in the case of a long electric railway, viz. that arising from actual leakage of the electricity due to defective insulation. To send an electric current through a distant motor, two wires, a "going" and "return" wire must be employed, insulated from one another by silk, guttapercha, or some insulating substance; and if the motor be on a moving train, there must be some means of keeping up continuous connection between the two ends of the moving electro-motor and the going and return wire. The simplest plan is to use the two rails as the two wires, and make connection with the motor through the wheels of the train; those on one side being well insulated from those of the other, otherwise the current would pass through the axles of the wheels, instead of through the motor. It is this simple plan that is employed in Siemens' Lichterfelde Electric Railway, now running at Berlin; the insulation arising from the rails being merely laid on wooden sleepers having been found sufficient for the short length, $1\frac{1}{2}$ mile. The car is similar to an ordinary tram-car, and holds twenty passengers. [Photographs were then projected on the screen of this and of the original electric railway laid by Siemens in the grounds of the Berlin Exhibition of 1879, and exhibited in 1881 at the Crystal Palace, Sydenham.] It was explained, that on this latter railway, which was 900 yards long, both the ordinary rails were used as the return wire, and that the going wire was a third insulated rail rubbed by the passing train. [Photographs were then projected on the screen of Siemens' electric tram-car at Paris, used to carry fifty passengers backwards and forwards last year to the Electrical Exhibition.] In this the going and return wires were overhead and insulated, connection being maintained between them and the moving car by two light wires attached to the car, and which pulled along two little carriages running on the overhead insulated wires, and making electric contact with them. [Experiments followed, proving that although two bare wires lying on the ground could be quite efficiently employed as the going and return wire, if the wires were short and the ground dry, the leakage that occurred if the wires were long and the ground moist was so great, as to more than compensate for the absence of the locomotive.]

Consequently Professor Perry and myself have for some time past been working out practical means for overcoming these difficulties, and we have arrived at what we hope is an extremely satisfactory solution. Instead of supplying electricity to one very long, not very well insulated rail, we lay by the side of our railway line a well-insulated cable, which conveys the main current. The rail, which is rubbed by the moving train, and which supplies it with electric energy, we subdivide into a number of sections, each fairly well insulated from its neighbour and from the ground; and we arrange that at any moment only that section or sections, which is in the immediate neighbourhood of the train, is connected with the main cable; the connection being of course made automatically with the moving train. As then leakage to the earth of the strong propelling electric current can only take place from that section or sections of the rail, which is in the immediate neighbourhood of the train, the loss of power by leakage is very much less than in the case of a single imperfectly insulated rail such as has been hitherto employed, and which being of great length, with its correspondingly large number of points of support, would offer endless points of escape to the motive current.

Dr. Siemens has experimentally demonstrated that an electric railway can be used for a mile or two; Professor Perry and myself, by keeping in mind the two essentials of success, viz. attention to both the mechanical and electrical details, have, we venture to think, devised means for reducing the leakage on the longest railway to less than what it would be on the shortest.

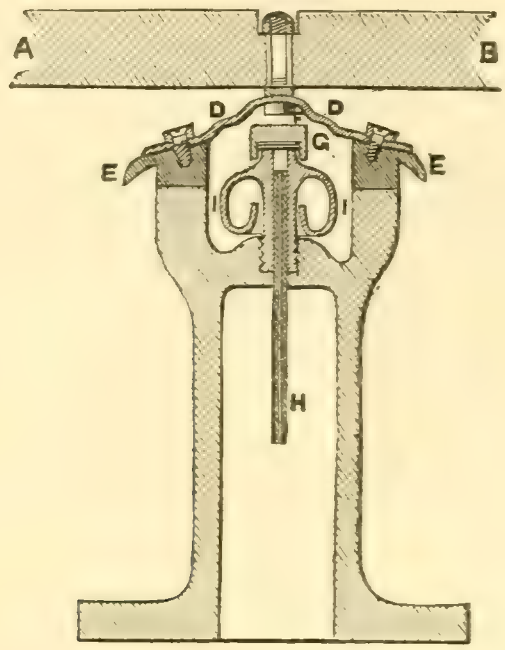
For the purpose of automatically making connection between the main well-insulated cable and the rubbed rail in the neighbourhood of the moving train we have devised various means, one of which is seen from the following figure.

A B is a copper or other metallic rod resting on the top of and fastened to a corrugated tempered steel disc D D (of the nature of, but of course immensely stronger than the corrugated top of the vacuum box of an aneroid barometer), and which is carried by and fastened to a thick ring E E made of ebonite or other insulating material. The ebonite ring is itself screwed to the circular cast-iron box, which latter is fastened to the ordinary railway sleepers. The auxiliary rail A B and the corrugated steel discs D D have sufficient flexibility that two or more of the latter are simultaneously depressed by an insulated collecting brush or roller carried by one or by all of the carriages. Depressing any of the corrugated steel discs brings the stud F, which is electrically connected with the rod A B, into contact with the stud G electrically connected with the well-insulated cable.

As only a short piece of the auxiliary rail A B is at any moment in connection with the main cable, the insulation of the ebonite ring E E will be sufficient even in wet weather, and the cast-iron box is sufficiently high that the flooding of the line or the deposit of snow does not affect the insulation. The insulation, however, of G, which is permanently in connection with the main cable, must be far

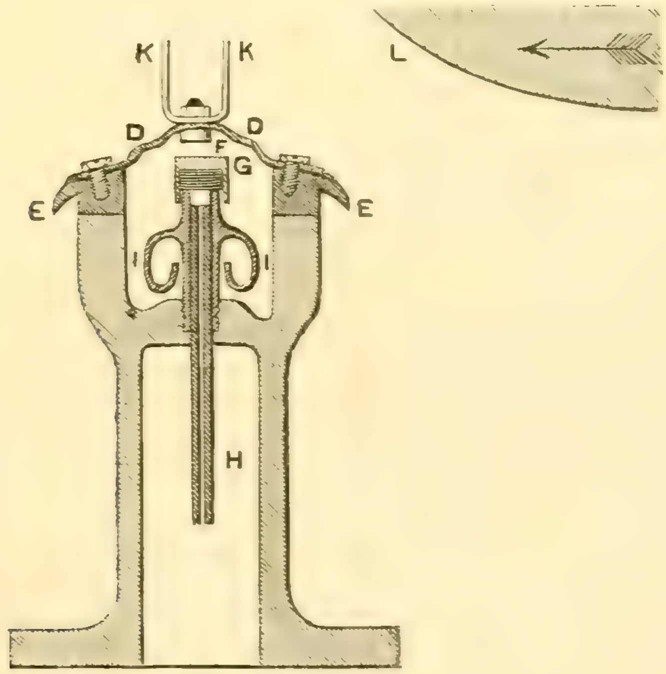
better. For this purpose we lead the guttapercha, or indiarubber, covered wire coming from the main cable through the centre of a specially formed telegraph insulator, and cause it to adhere to the

Fig. 1.



inside of the earthenware tube forming the stalk. And as, in addition, the inside of each contact box is dry, a very perfect insulation is maintained for the lead coming from the main cable. Consequently as all leakage is eliminated except in the immediate neighbourhood

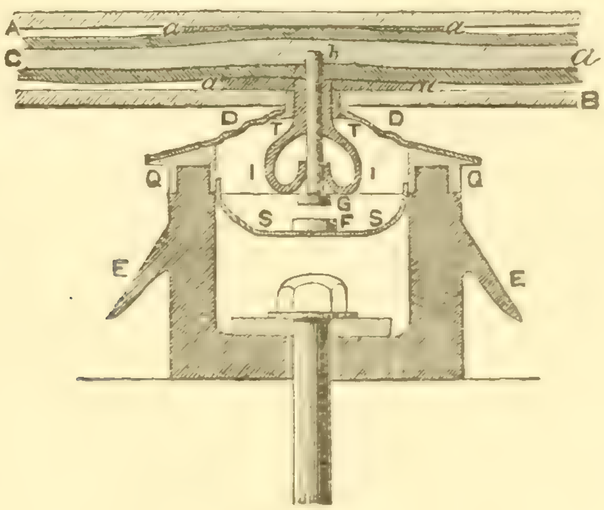
Fig. 2.



of the train, this system can be employed for the very longest electric railways. Fig. 2 shows a modification of the contact box when the insulated rail instead of extending all along the line is quite short

and is carried by the train, and by its motion presses forwards and downwards a metallic fork on the contact box, thus making contact between F and G. [Other diagrams were explained, illustrating modifications of the contact boxes, in one case the well-insulated cable is carried inside the flexible rail, which then takes the form of a tube, shown in Fig. 3, in another case the cable is insulated with

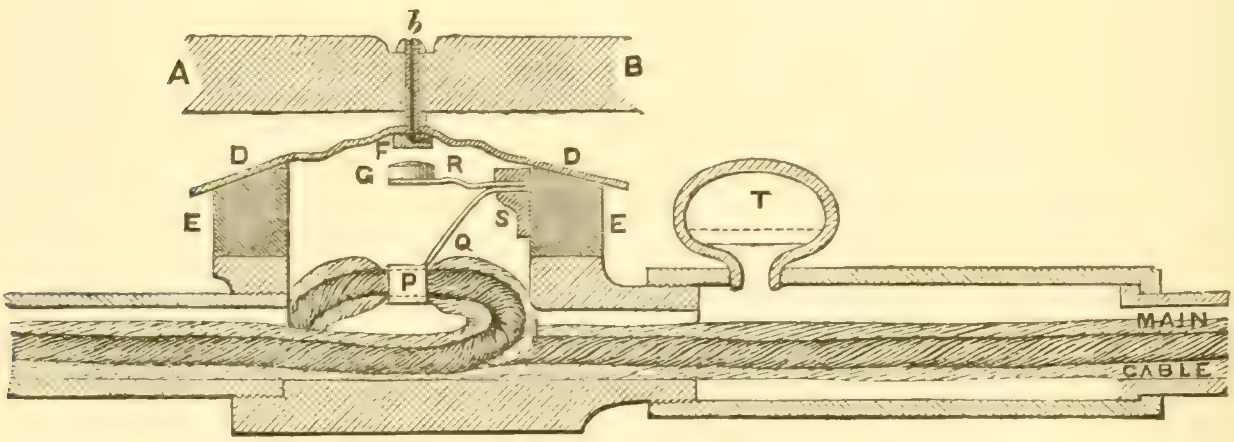
FIG. 3.



paraffin oil instead of with guttapercha or indiarubber, shown in Fig. 4, &c.]

The existence of these contact boxes at every 20 to 50 feet also enables the train to graphically record its position at any moment on a map hanging up at the terminus, or in a signal-box or elsewhere, by a shadow which creeps along the map of the line as the train advances;

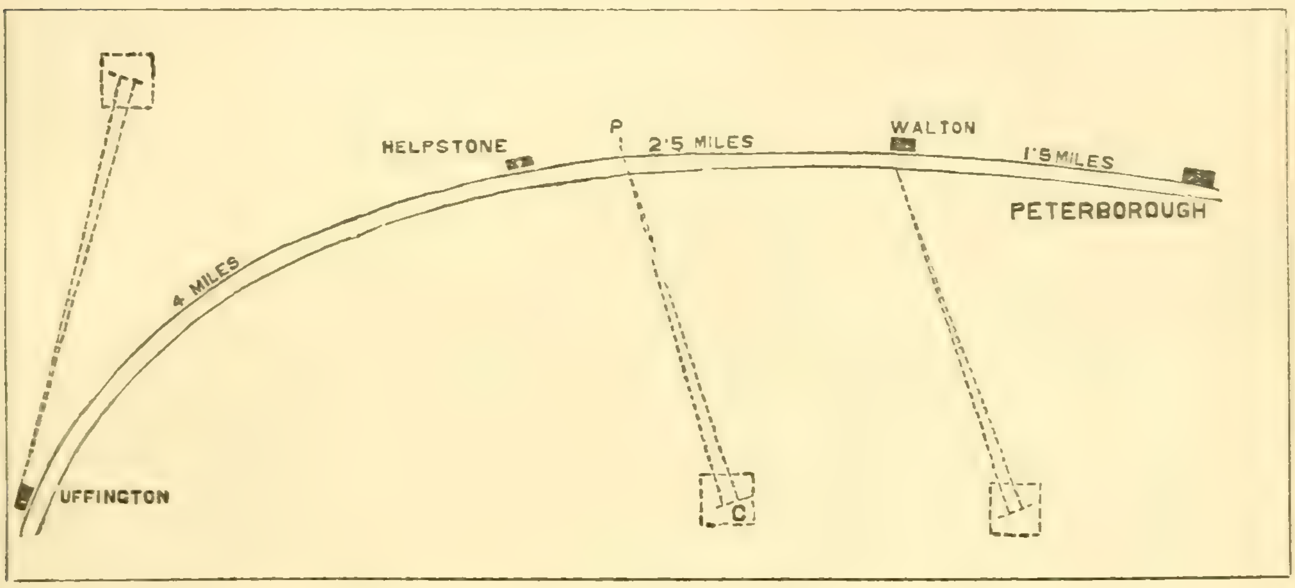
FIG. 4.



stops when the train stops; and backs when the train backs. This is effected thus:—as the train passes along, not only is the main contact between F and G automatically made, as already described, but an auxiliary contact is also completed by the depression of the lid of the contact box, and which has the effect of putting, at each contact box in succession, an earth fault on an insulated thin auxiliary wire running by the side of the line. And just as the position of an earth fault can be accurately determined by electrical testing at the end of the

line, so we arrange that the moving position of the earth fault, that is the position of the train itself, is automatically recorded by the pointer of a galvanometer moving behind a screen or map, in which is cut out a slit representing by its shape and length the section of the line on which the train is, as shown in Fig. 5. In addition, then, to the small sections of 20 feet or more into which our auxiliary rubbed rail is electrically divided, there would be certain long blocked sections one mile or several miles in length, for each of which on the map a separate galvanometer and pointer would be provided. [Experiments were shown of the system of graphically automatically recording the progress of a train.]

Fig. 5.



In the preceding systems there are several contact-boxes in each section of the insulated rubbed rail, and several sections of the insulated rail in each section of the line blocked, but in the next system the rubbed rail is simply divided electrically into long sections each of as great a length as the particular system employed to insulate the rubbed rail will allow. In this case we arrange that the electric connection between the main cable and the rubbed conductor shall be automatically made by the train as it enters a section, and automatically broken as the train leaves a section. The model before you, shown in the accompanying figure, is divided into four sections, each about 11 feet in length, and you see from the current detectors that as the train runs either way it puts current into the section just entered, and takes off current from the section just left.

[Experiments were then shown of the ease with which an electric train could be made to back instead of going forwards, by reversing the connections between the revolving armatures and the fixed electro-magnets of the motor; also that the accidental reversal of the field magnets of the main stationary generator, although it had the effect of reversing the main current, produced no change in the direction of motion of an electric engine, the direction of motion being solely under the control of the driver.]

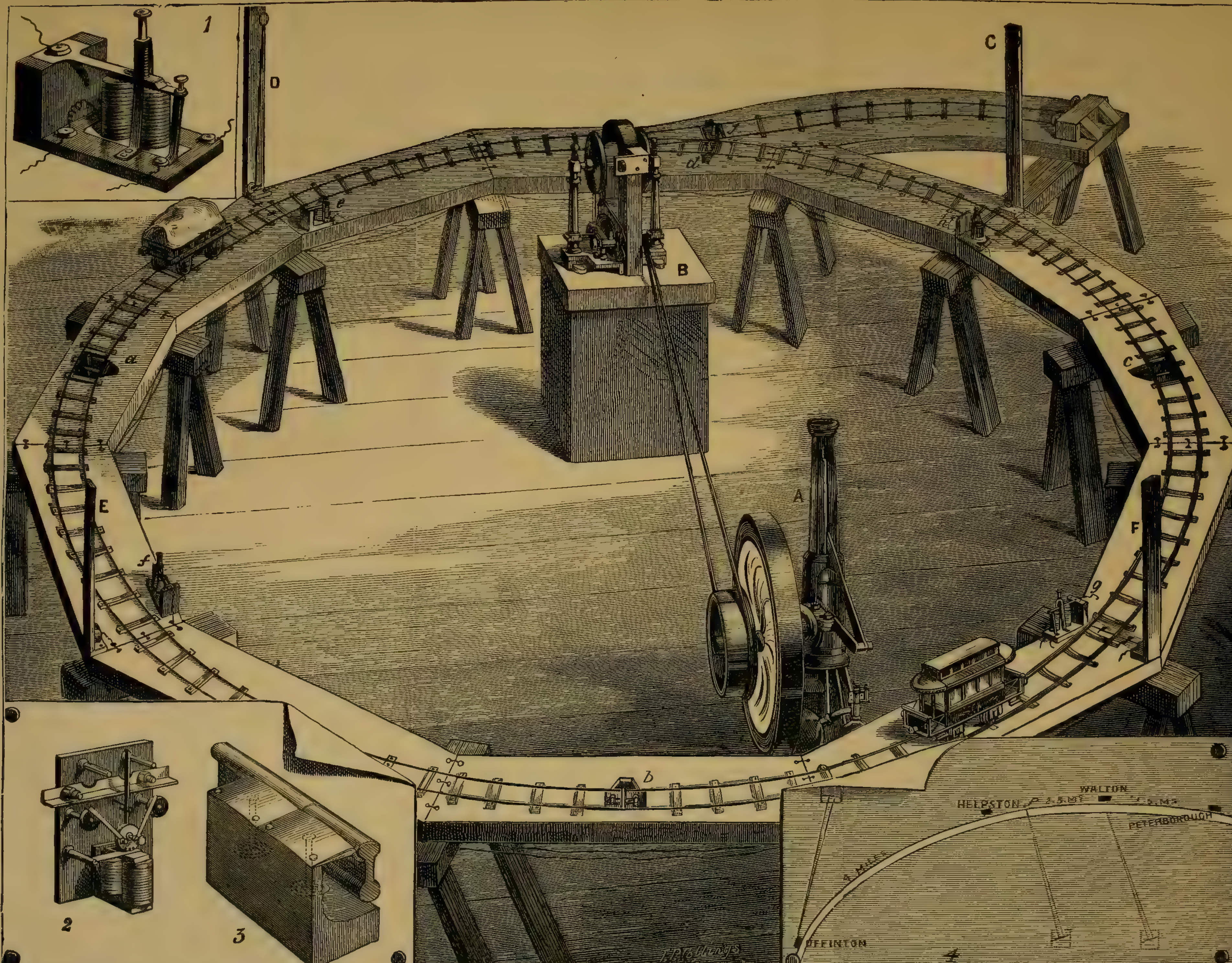
But more than this, not only does the train take off current from the section 1 when it is just leaving it, and entering section 2, but no following train entering section 1 can receive current or motive power until the preceding train has entered section 3. [Experiments were then shown proving that with this system a following train could not possibly run into a preceding train even if the preceding train stopped or backed.] Now why does the following train when it runs on to a blocked section pull up so quickly? The reason is because it is not only deprived of all motive power, but is powerfully braked, since when electricity is cut off from a section the insulated and non-insulated rail of that section are automatically connected together, so that when the train runs on to a blocked section the electro-motor becomes a generator short circuited on itself, producing, therefore, a powerful current which rapidly pulls up the engine. [Experiments were then shown of the speed with which an electro-motor, which had been set in rapid rotation and then deprived of its motive current, pulled up when its two terminals were short circuited.]

Whenever, then, a train, it may be even a runaway engine, enters on a blocked section, not only is all motive power withdrawn from it, but it is automatically powerfully braked, quite independently of the action of the engine-driver, guard, or signalman. No fog, nor colour-blindness, nor different codes of signals on different lines, nor mistakes arising from the exhausted nervous condition of overworked signal-men, can with this system produce a collision. The English system of blocking is merely giving an order to stop a train; but whether this is understood or intelligently carried out is only settled by the happening or non-happening of a subsequent collision. Our Absolute Automatic Block acts as if the steam were automatically shut off and the brake put on whenever the train is running into danger; nay, it does more than this—it acts as if the fires were put out, and all the coal taken away, since it is quite out of the power of the engine-driver to re-start his train until the one in front is at a safe distance ahead.

But all trains will undoubtedly be lighted with electricity; must, then, the train be plunged into darkness when it runs on to a blocked section to which no electric energy is being supplied? No! If some of the electric energy supplied to the train when it is on an unblocked section be stored up in Faure's accumulators, such as are at present used on the Brighton Pulman train, the lamps will continue burning even when the train has ceased to receive electric energy from the rubbed rail.

When, then, we commit the carrying of our power to that fleet messenger to which we have been accustomed to entrust the carrying of our thoughts, then shall we have railways that will combine speed, economy, and safety; and last, but not least to us Londoners, we shall have the entire absence of smoke, the presence of which nearly causes the convenience of the Underground Railway to be balanced by the pernicious character of its atmosphere.

[W. E. A.]



A, Gas Engine supplying Motive Power; B, Magneto Electric Machine; C, D, E, F, Electric Current Indicators; *a, b, c, d,* Contact Makers at the ends of each Section; *e, f, g, h,* Blocking Electro-Magnets attached to each Section; 1, Blocking Electro-Magnet (enlarged); 2, Contact Makers as in Model; 3, Contact Models as required for actual use; 4, Map Indicator, automatically showing the position of Trains on the Line.

[*To face p.* 74.

WEEKLY EVENING MEETING,

Friday, March 31, 1882.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

W. Spottiswoode, Esq. LL.D. Pres. R.S. *M.R.I.*

Matter and Magneto-Electric Action.

The late Professor Clerk Maxwell, in his work on 'Electricity and Magnetism' (vol. ii. p. 146), lays down as a principle that "the mechanical force which urges a conductor carrying a current across the lines of magnetic force, acts, not on the electric current, but on the conductor which carries it. If the conductor be a rotating disk or a fluid it will move in obedience to this force, and this motion may or may not be accompanied with a change of position of the electric current which it carries. But if the current itself be free to choose any path through a fixed solid conductor or a network of wires, then, when a constant magnetic force is made to act on the system, the path of the current through the conductors is not permanently altered, but after certain transient phenomena, called induction currents, have subsided, the distribution of the current will be found to be the same as if no magnetic force were in action. The only force which acts on electric currents is electromotive force, which must be distinguished from the mechanical force which is the subject of this chapter."

In the investigation on electric discharges, on which Mr. Moulton and myself have been long engaged, we have met with some phenomena of which the principle above enunciated affords the best, if not the only, explanation. But whether they be regarded as facts arising out of that investigation, or as experimental illustrations of a principle laid down by so great a master of the subject as Professor Clerk Maxwell, I have ventured to hope that they may possess sufficient interest to form the subject of my present discourse.

The experiments to which I refer, and of which I now propose to offer a summary, depend largely upon a special method of exciting an induction coil. This method was described in two papers, published in the 'Philosophical Magazine' (November 1879) and in the 'Proceedings of the Royal Society' (vol. xxx. p. 173), respectively; but as its use appears to be still mainly confined to my own laboratory, and to that of the Royal Institution, I will, with your permission, devote a short time to a description of it, and to an exhibition of its general effects.

The method consists in connecting the primary circuit directly with a dynamo- or magneto-machine giving alternate currents. In the present case, I use one of M. de Meritens' excellent machines

driven by an Otto gas engine. The speed of the de Meritens' machine, so driven, is about 1100 revolutions per minute.

In this arrangement the currents in the secondary are of course alternately in one direction and in the other, and equal in strength; so that the discharge appears to the eye, during the working of the machine, to be the same at both terminals.

The currents in the primary are also alternately in one direction and in the other, and consequently, at each alternation, their value passes through zero. But they differ from those delivered in the primary coil with a direct current and contact breaker in an important particular, namely, that while the latter, at breaking, fall suddenly from their full strength to zero, and then recommence with equal suddenness, the former undergo a gradual although very rapid change from a maximum in one direction through zero to a maximum in the opposite direction. The ordinary currents with a contact breaker would be represented by a figure of this kind,

while those from the alternate machine approximately by a curve of the following form.

The rise and fall of the latter are, however, sufficiently rapid to induce currents of high tension and of great quantity in the secondary.

From these considerations it follows: first, that as the machine effects its own variations in the primary current, no contact breaker is necessary; secondly, that as there is no sudden rupture of current, there is no tendency in the extra current to produce a spark or any of the inconveniences due to an abrupt opening of the circuit, and consequently that the condenser may be dispensed with; thirdly, that the variations in the primary, and consequently the strength and period of delivery of the secondary currents are perfectly regular; fourthly, that the strength of the currents in the secondary is very great. With a 26-inch coil by Apps I have obtained a spark about 7 inches in length, of the full thickness of an ordinary cedar pencil. But for a spark of thickness comparable at least with this, and of 2 inches in length, an ordinary 4-inch coil is sufficient.

Owing to the double currents, the appearance of the discharge is that of a bright point at each terminal, and a tongue of the yellow flame, such as is usually seen with thick sparks from a large coil, issuing from each. This torrent of flame (which, owing to the rapidity with which the currents are delivered by the machine, is apparently continuous) may be maintained for any length of time. The sparks resemble those given by my great coil (exhibited in this theatre on Friday, April 13th, 1877, and described in the 'Philosophical Magazine,' 1877, vol. iii. p. 30) with large battery-power and with a mercury break; but with that instrument it is doubtful whether such thick sparks could be produced at short intervals, or in a rapid shower, as in this case.

In order to contrast the effects of the two methods, I will excite the coil, first with a battery, and secondly with the alternating

machine. You will notice that with the battery we can obtain either long, bright, and thin sparks, or short and comparatively thick discharges; but, unless the latter are made very short, they occur only at comparatively long and even perceptible intervals of time. On the other hand, with the alternate machine, although the method does not lend itself so readily to the production of long and bright sparks, we can produce a perfect torrent of discharges more rapid and more voluminous than by any other means yet devised. Long bright sparks can, however, be obtained by interrupting the flow of the currents from the machine, and by allowing only single currents to pass at comparatively long intervals. It may be interesting to know that the number of currents given out by the machine, and consequently the number of discharges issuing from the coil, is no less than 35,200, that is, 17,600 in each direction, per minute. The number may be determined by the pitch of the note which always accompanies the action of an alternate machine.

A comparison of the two methods may also be made when a Leyden jar is used as a secondary condenser. This application of the jar is well known as a valuable aid in spectroscopic research; and the employment of the alternating machine so materially heightens the effects that, judging from some experiments made in the presence of Mr. Lockyer, and from others of a different character in the presence of Professor Dewar, I am led to hope from it a further extension of our knowledge in this direction. In order that you may form, at all events, some rough idea of the nature of such discharges, I venture, at the risk of causing some temporary inconvenience from the noise, to project the spectrum of this spark.

I will detain you with only one more instance of comparison. The ordinary effect of an induction coil in illuminating vacuum tubes is well known. The result is usually rather unsteady. Several instruments have been devised to obviate this inconvenience, e. g. the rapid breakers described in the 'Proceedings' of the Royal Society (vol. xxiii. p. 455, and vol. xxv. p. 547), or the break called the "Trembleur" of Marcel Deprez (see 'Comptes Rendus,' 1881, I. Semestre, p. 1283). The use of the alternating machine, however, not only gives all the regularity in period, and uniformity in current, aimed at in these instruments, but also at the same time supplies currents of great strength. The result is a discharge of great brilliancy and steadiness, and it is perhaps not too much to say that the effects are comparable to those obtained with Mr. De La Rue's great chloride of silver battery. The configuration of the discharge produced in this way can also be controlled by a suitable shunt applied to the secondary circuit; for example, one formed by a column of glycerine and water, or the one consisting of a film of plumbago spread upon a slab of slate, constructed by my assistant Mr. P. Ward, and here exhibited.

One test of the strength of current passing through a tube is the amount of surface of negative terminal which it will illuminate with a bright glow. I here have a tube with terminals in the form

of rings, each of which would be regarded of ample size for currents obtained in the ordinary way. These are now all connected together so as to form one grand negative terminal; and it will be found that with the currents from the alternate machine the whole system is readily illuminated at once.

It should perhaps be here remarked that, while the strength of the secondary currents passing through the tube is partly due directly to the strength of the primary currents from the machine, it is probably also in part due to the rapidity with which the secondary currents follow one another. Owing to the latter circumstance the column of gas maintains a warmer and more conductive condition than would prevail if the interval between the discharge was longer; and in consequence of this a larger portion of the discharges can make its way through than would otherwise be the case.

Before leaving the instrumental part of my discourse, I desire to bring under your notice a modification of the machine which we have thus far used for producing, by the intervention of the induction coil, currents of high tension. This consists of a machine of the same general construction as the other, but having the armatures wound with a much greater number of convolutions of much finer wire. The result is a machine giving off currents of sufficient tension to effect, by direct action, discharges through vacuum tubes, and even in air. The currents are of course alternate; but by diminishing the size of one of the terminals to a mere point, as well as by other methods described elsewhere, it is possible to shut off the currents in one direction, leaving only those in the other direction to discharge themselves through the tube. I hope on some future occasion to give a fuller account of this remarkable machine, which has only quite recently been completed.

Returning to the discharge in air, it will be noticed that when the terminals are set horizontally the torrent of thick discharges assumes the appearance of a flame, which takes the form of an inverted V. This is the result of convection currents due to the heat given off by the discharges themselves. The discharges are by their nature as it were fixed at each end, but within the limits of discharging distance free to move about and to extend themselves in space, especially in their central part. Further, it may be observed that the length of the spark which can be maintained is greater than that over which it will leap in the first instance. The explanation of this is to be sought in the fact that when the sparks follow very rapidly in succession, the whole path of each discharge remains so far in a heated state as to assist the passage of the next; and, further, that in the middle part of the discharge or apex of the Λ, where the heat is greatest, the heat prevails to such an extent as to render a portion of the path highly conductive. This may be illustrated by holding a gas jet near the path of the discharge. The flames will then leap to the two ends of the jet, which will perform the part of a conductor; and the real length of the discharge will be that traversed from terminal to

terminal, minus the length of the intervening flame. The permanently heated part of the flame will act in the same manner in extending the effective length of the discharge.

The discharge which we are now examining is not homogeneous throughout, but consists of more than one layer. The flame, which, from the fact of its forming the outer sheath of the discharge, is the most prominent feature, consists mainly of heated but solid particles emanating from the terminals. That this is the case may be inferred in a general way from the colours which the flame assumes when different substances are placed upon the terminals; for example, lithium or sodium. The spectrum of the flame appears to be always continuous. A convenient substance to affix to the terminals is boron glass, on account of the brilliancy to which it gives rise in the discharge; this will enable us to project the phenomenon. Within this sheath of flame, the discharge consists of the pink light characteristic of air, and in the centre of all the true bright spark. There is reason to think that, under certain circumstances, there are more layers to be seen; but the above division is sufficient for our present purpose. In this somewhat complicated structure, the pink light corresponds to the arc, and the flame to a similar accompaniment which is seen playing about the upper carbon in electric lamps when a current of great strength is used.

From this account of the methods here employed I now turn to the main question. In the investigation, to which allusion was made at the beginning of this lecture, it occurred to us that an examination of the effects of a magnetic field on discharges of this character through air or other gases at atmospheric pressure, and a comparison with those obtained at lower pressures, might throw some fresh light on the nature of electrical discharges in general. It is these phenomena to which I now propose to ask your attention.

When the discharge, originally in the form of a vertical spindle, is submitted to the action of a magnet whose poles are horizontal, it spreads out into two nearly semicircular disks, one due to the discharges in one direction, and the other to those in the opposite direction. As the magnetism is strengthened, the flame retreats towards the edge of the disks, and ultimately disappears. The disk then consists mainly of the pink discharge; but with a still stronger magnetic field, it is traversed at intervals by bright semicircular sparks at various distances from the centre. In every case, bright sparks pass directly between the terminals at the opening of each separate discharge.

In order further to disentangle the parts of this phenomenon, recourse was had in the original experiments to a revolving mirror. The light in the disks is insufficient to allow of a projection of the effects, but the accompanying diagrams represent the appearances seen in the mirror. Fig. 1 shows the arrangement of the terminals and the magnetic poles; Fig. 2 the appearance of the discharges in a plane at right angles to that of Fig. 1; Fig. 3 the appearance of three

Fig. 1.

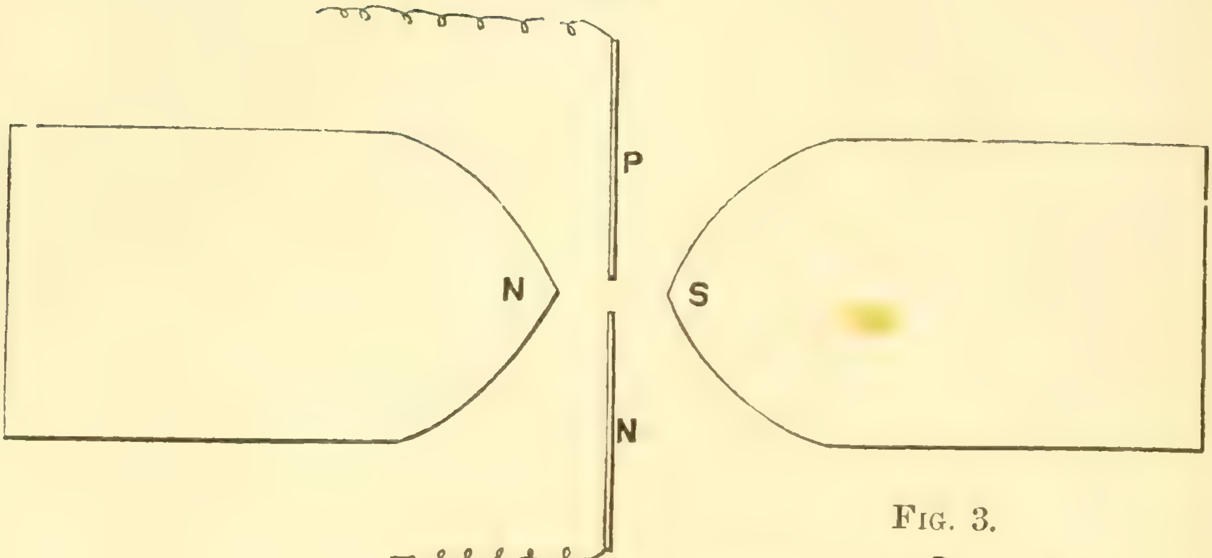


Fig. 3.

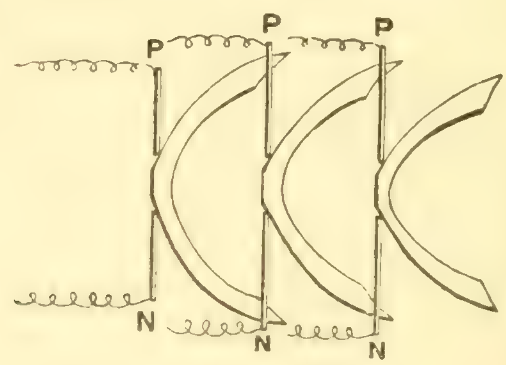


Fig. 2.

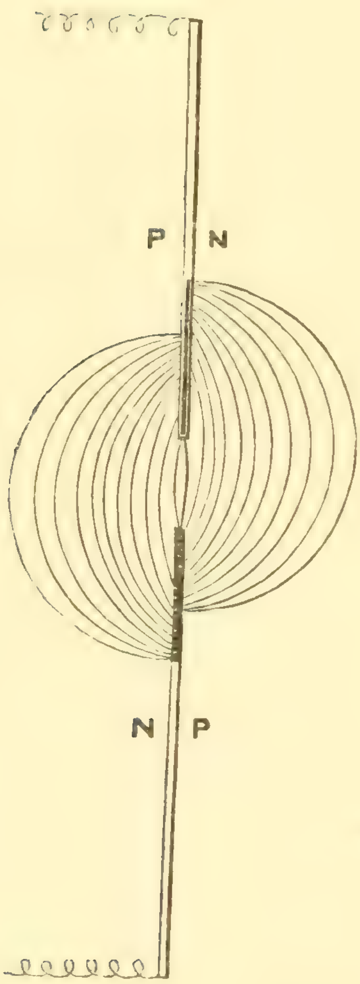


Fig. 4.

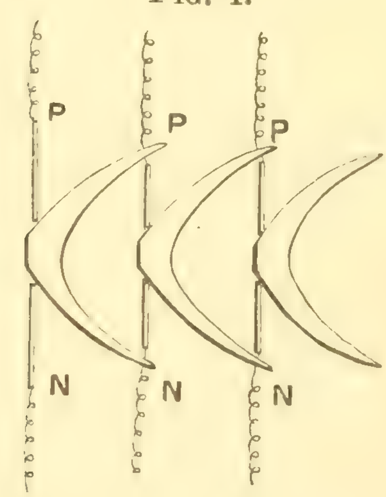


Fig. 6.

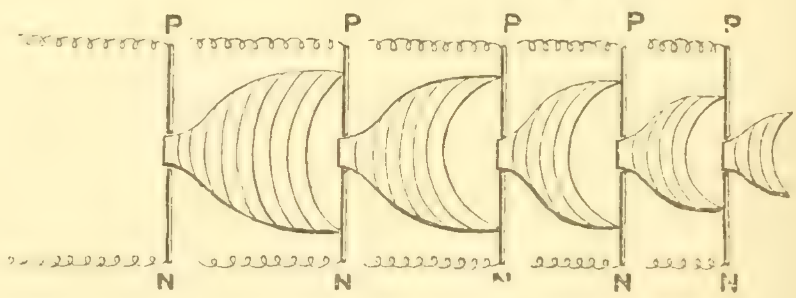


Fig. 5.

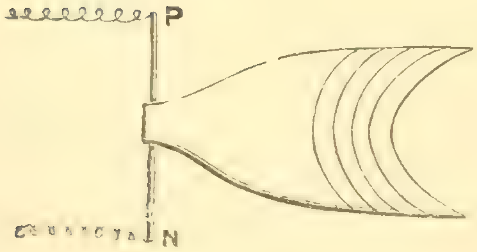
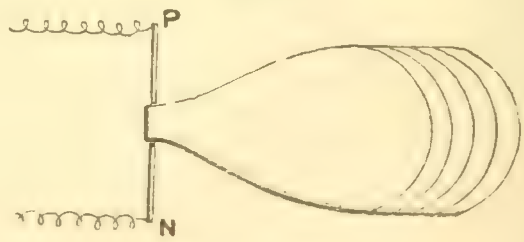


Fig. 7.



successive discharges (in the same direction) with a weak magnetic field and a slowly revolving mirror; Fig. 4 the same, with a slightly more rapid rate of revolution; Fig. 5 a single discharge, with a stronger field and greater speed of mirror; Fig. 6 a single discharge in a strong field, with a still greater speed of mirror. It should be mentioned that in all these figures the images to the left are to be regarded as anterior to those on the right, and that they represent various phases of the left-hand discharge in Fig. 2.

If, however, we observe the right-hand discharges with a mirror revolving in the same direction as before, it is clear that the actual curvature of the discharge will be turned in the opposite direction (with reference to the motion of the mirror) to that in the case of the left-hand discharges. The consequence will be that the appearance in the mirror, when the rate of revolution is not too great, will be something like Fig. 7, instead of Fig. 6. As the speed of the mirror is increased, the convexity will diminish, and ultimately be replaced by a concavity of the same kind, although not so marked, as that in the case of the left-hand discharges.

These diagrams show that each coil discharge commences with a bright spark passing directly between the terminals; that this spark is in general followed by the pink light or arc discharge, which passes first in the immediate neighbourhood of the initial spark, and gradually extends like an elastic string in semicircular loops outwards; and that the flame proper is a phenomenon attendant on the close of the entire discharge. It should be added that observations with a mirror revolving on a horizontal axis, and with a horizontal slit in front of the discharge, show that the disk is not simultaneously illuminated throughout, but that it is a locus of a curvilinear discharge which moves outwards and expands in its dimensions from the centre.

The mechanism of the discharge would therefore seem to be as follows: In the first place, as soon as the tension is sufficient, the electricity from the terminals breaks through the intervening air, but with such rapidity that the fracture is like that of glass, or other rigid substance. This opens a path, along which, if there remains sufficient electricity of sufficient tension, the discharge will continue to flow. During such continuance the gas becomes heated, and behaves like a conductor carrying a current; and upon this the magnet can act according to known laws. As long as the electricity continues to flow, the heat will at each moment determine the easiest, although not the shortest path for its subsequent passage. In this way the gas, which acts at one moment as the conductor of the discharge, and at the next as the path for it, will be carried further and further out until the supply of the electricity from the coil fails, and the whole discharge ceases. We are, in fact, led by these experiments to the conclusion that it is the gas in the act of carrying the current, and not the current moving freely in the gaseous space, upon which the magnet acts.

This explanation of the magnetic displacement of a discharge

receives strong support from the phenomena represented in Figs. 5, 6, and 7. The successive bright lines there shown must be due to successive falls and revivals of tension within a single coil discharge. The existence of such alternations in coil discharges of large quantity is otherwise known. When the fall in temperature is such that the conductivity of the gas is insufficient to maintain the arc, the discharge can make its way through the air only by a fresh rent of the same kind as the first fracture. But how can this be reconciled with the fact that the tension can never reach its original degree, and must, on the whole, be gradually falling, and that, in addition, the paths represented by these various sparks are successively longer and longer? The answer to this question is to be found plainly written in the phenomena themselves. Any irregularity in one of these bright lines is always found to be accurately repeated in all of the same series. Now, it is scarcely to be conceived that, at successive instants of time and in different portions of space, irregularities in the discharge itself, and in the distribution of the gas, so precisely the same, would constantly and for certain recur; and we are therefore driven to the conclusion that it is the same portion of gas which at first occupied the centre of the field, with its same yet unhealed rent, which is moved outward under the action of the magnet. If this be so, we have in this repetition of minute details nothing more than what would necessarily follow from successive reopenings of the weak parts of the gas, which would be surely found out by the electricity in its struggle to pass.

The view here taken of the material character of the luminous discharge is further borne out by the fact that the spindle of light is capable of being diverted by a blast of air. When the blast is gentle, the discharge becomes curvilinear, approximately semicircular, and the yellow flame may be seen playing about the outer edge, in the same way as in a weak magnetic field. When the blast is stronger, the sheet of light becomes irregular in form, and it is traversed by a series of bright lines, all of which follow, even in their minute details, the configuration of the sheet. The analogy between this and the phenomena produced in a strong magnetic field needs no further remark. If the strength of the blast be still further increased, the flame and the sheet of light both disappear, and nothing remains but bright sparks passing directly, and undisturbed, between the terminals. In this case the air is both displaced and cooled so rapidly by the blast, that it no longer offers a practicable conductive path for the remainder of the electricity, coming from the coil, to follow. Of this a succession of disruptive sparks is a necessary consequence.

The effect thus produced by a very strong blast is in fact similar to that observed when a jar is used as a secondary condenser. In this case the electricity, instead of flowing gradually from the coil, passes in one or more instantaneous discharges with finite intervals of time between them. Each of these has to break its way through

the air; and, that done, it ceases. Hence, neither a magnet, nor a blast of air will have any effect in diverting such a discharge.

As a last stage of the phenomena, it may be mentioned that, if the interval between the terminals be near the limit of striking distance, either a blast of air, or the setting up of a magnetic field will alike extinguish the discharge.

Our experiments have been thus far carried on in air at atmospheric pressure; but there is nothing in this pressure which is essential to them or to the conclusions to which we have been led. We may therefore repeat them in air, or any other gaseous medium, at any pressure we please. This consideration leads us into the region (so fertile in an experimental point of view) of discharges in vacuum tubes.

Commencing with a tube of moderate diameter and of very slight exhaustion, we can at once recognize our former phenomena slightly changed. Proceeding to another tube, of larger diameter and of moderate exhaustion, and placing it axially or equatorially in a magnetic field, we see not only that the discharge (or rather the conductor carrying it) is displaced, but also that the displaced part is spread out into a sheet or ribbon, showing that the discharge is affected gradually, exactly in the same way as was found in the open air.

When the exhaustion is carried further, the phenomena become rather more complicated. At an early stage there is a distinct separation between the "negative glow" and the rest of the luminous column; and at a more advanced stage the column itself is broken into separate luminosities or striæ. When this is the case, it is usually said that the negative glow follows the lines of magnetic force, while the luminous column distributes itself according to Ampère's law.

It will, however, be found that when completely analysed the action of the magnet upon the striæ, taken individually, is the same as that upon the negative glow, due allowance being made for the differences in local circumstances subsisting between the one and the other. We have elsewhere shown that the negative glow is in reality as truly a stria as any other individual member of the luminous column; but with this difference, that it is anchored to, and dependent for its form on, a rigid metallic terminal, whereas each of the others is dependent on the variable form and position of the stria immediately next in order, reckoning from the negative end of the tube. The action of a magnet in throwing the negative glow into a sheet of light, which is the locus of the lines of force passing through the terminal, and which consequently varies with the position of the tube in the field, is a phenomenon so well known that we need repeat only a single experiment by way of reminder.

Although it is not altogether so easy to show that the other striæ are directly affected by a magnetic field in the same way as is the anchored stria, we may still satisfy ourselves that it is the fact, from the consideration that when the striæ are well developed and the

magnetic field is strong, it is quite possible to form a magnetic arch at any part of the column. In this experiment it will be noticed that for the formation of the arch in mid-column it is necessary that both poles of the magnet should act upon one and the same stria. This, in fact, means that the pole nearest the negative end anchors the stria, and thereby brings it into conditions similar to those of the negative glow. When this is effected the two exhibit similar modifications in the magnetic field.

In support of this view, we may adduce another and quite independent method of anchoring a stria, and of thereby producing a magnetic arch elsewhere than at the negative terminal. It was noticed by Goldstein and others that if the negative terminal of a tube be enveloped by an insulating surface of any form pierced with a number of holes, or if a diaphragm similarly pierced be placed anywhere in the tube, that the pierced surface will act as a negative terminal. He also found that the finer and closer the holes, the more complete the resemblance to the action of a negative terminal. But even when the substance is metallic, and when the holes are neither very small nor very numerous, a perforated diaphragm will so far act like a negative terminal as to serve as a point of departure of a stria. There is, however, this difference, that the blank space immediately adjoining the diaphragm, as it is usually called, is not generally so large as that at the true terminal; and the striæ thus artificially formed always lie close up to the holes. The diaphragm, in fact, anchors the stria, and renders it susceptible of the same magnetic effect as was shown in the cases studied before.

The action of a diaphragm in a magnetic field gives rise to many other interesting and remarkable results; some of which would further illustrate the views now submitted for your consideration. But these must be reserved for another occasion.

In the foregoing experiments, and in the remarks which have accompanied them, I have endeavoured to illustrate, by reference to gaseous media, the principle enunciated at the outset, that in the displacement of the discharge in a magnetic field, the subject of the magnetic action is the material substance or medium which conveys the discharge. I have shown also that, even when the discharge takes place in media so attenuated as to produce the phenomena of striæ, the same principle applies not only to the discharge as a whole, but also to each component stria or unit; and, lastly, that the apparent diversity of effect on the various striæ is due to local circumstances, and not to any fundamental difference between the "negative glow" and the members of the "positive column."

Seeing now that the magnetic displacement of the luminous discharge means displacement of the matter in a luminous condition, and that a crowding of such luminous matter involves an increase of luminosity, may we not infer with a high degree of probability that the striæ are themselves aggregations of matter, and that the dark spaces between them are comparatively vacuous.

It is true that such a view of the case would seem to imply that, in gaseous media, the better the vacuum the more easily can the electricity pass; and that this might at first sight appear to be at variance with the known fact that the resistance of a tube decreases with the pressure until a minimum, determinate for each kind of gas, and then increases. But it has been suggested by Edlund ('Annales de Chemie et de Physique,' 1881, tom. iii. p. 199) that the resistance of a tube may really consist of two parts, first that due to the passage of the electricity through the gas itself and, secondly, that due to its passage from the terminals to the gas; and also that the former decreases, while the latter increases, as the pressure is lowered. On this supposition the observed phenomena may be explained, without assigning any limit to the facility with which electricity may traverse the most vacuous space.

We may even carry the suggestion of a resistance of the second kind a little further, and suppose that there is a resistance due to the passage of electricity from a medium of one density to that of another, or from layer to layer of different degrees of pressure. And from this point of view we may regard the striæ as expressions of resistance due to the varying pressure in different parts of the tube. Into the question, whence this variation of pressure, I am not at present prepared to enter; it must suffice for this evening to have shown that the conclusions which we have drawn from our experiments are not in disaccordance with other known phenomena of the electrical discharge.

[W. S.]

GENERAL MONTHLY MEETING,

Monday, April 3, 1882.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Benjamin Baker, Esq. M.I.C.E.
William Edmund Rich, Esq. M.I.C.E.

were elected Members of the Royal Institution.

Eleven Candidates for Membership were proposed for election.

The Special Thanks of the Members were returned to Edward J. Muybridge, Esq. for his discourse on Attitudes of Animals in Motion on March 13th.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor General of India—Geological Survey of India:
Records. Vol. XV. Part 1. 8vo. 1882.
Palæontologia Indica: Series II. XI. XII. Vol. 3. Series XIII. Vol. 1. 4to. 1881.
Manual of the Geology of India. Part III. 8vo. 1881.
Memoirs. Vol. XVIII. Parts 1–3. 8vo. 1881.
The Secretary of State for India—Synopsis of the Great Trigonometrical Survey of India. Vols. X. XI. XII. and XIII. 4to. 1880.
Accademia dei Lincei, Reale, Roma—Atti, Serie Terza: Vol. VI. Fasc. 7, 8. 4to. 1882.
Astronomical Society, Royal—Monthly Notices, Vol. XLII. No. 4. 8vo. 1882.
Bankers' Institute—Journal, Vol. III. Part 3. 8vo. 1882.
Barlow, Peter W. Esq. M.I.C.E. F.R.S. F.G.S. (the Author)—Smoke Abatement. 8vo. 1882.
Bramwell, Sir Frederick J. F.R.S. M.R.I. (the Author)—On Some of the Developments of Mechanical Engineering during last half century. 8vo. 1882.
Railways and Locomotives (Lectures in 1877), by J. W. Barry and F. J. Bramwell. 8vo. 1882.
British Architects, Royal Institute of—Proceedings, 1881–2, Nos. 10, 11, 12. 4to. 1881–2.
Chemical Society—Journal for March, 1882. 8vo.
Index of Vols. XXXIX. and XL.
East India Association—Journal, Vol. XIV. No. 4. 8vo. 1882.
Editors—American Journal of Science for March, 1882. 8vo.
Analyst for March, 1882. 8vo.
Athenæum for March, 1882. 4to.
Chemical News for March, 1882. 4to.
Engineer for March, 1882. fol.
Horological Journal for March, 1882. 8vo.
Iron for March, 1882. 4to.
Nature for March, 1882. 4to.
Revue Scientifique and Revue Politique et Littéraire for March, 1882. 4to.
Telegraphic Journal for March, 1882. fol.
Franklin Institute—Journal, No. 675. 8vo. 1882.
Geneva: Société de Physique et d'Histoire Naturelle—Mémoires. Tome XXVII. Partie 2. 4to. 1881.
Geographical Society, Royal—Proceedings, New Series, Vol. IV. No. 4. 8vo. 1882.
Geological Society—Abstracts of Proceedings, 1881–2, Nos. 417–418. 8vo.

Gordon, Charles Alexander, M.D. C.B. M.R.I. (*the Author*)—Life on the Gold Coast. 8vo. 1874.
Our Trip to Burmah. 8vo. 1876.
Guy, William A. Esq. F.R.S. &c. (*the Author*)—John Howard's Winter's Journey. 12mo. 1882.
Linnean Society—Journal, No. 91. 8vo. 1881–2.
Transactions: Botany, Vol. II. Part 1. Zoology, Vol. II. Parts 2–4. 4to. 1881–2.
Manchester Geological Society—Transactions, Vol. XVI. Part 13. 8vo. 1882.
McKendrick, John Gray, M.D. F.R.S.E. (*the Author*)—Outlines of Physiology in its Relations to Man. 12mo. 1878.
Meteorological Office—Report of the Meteorological Council of the Royal Society for 1880–1. 8vo. 1882.
Newcastle-on-Tyne Literary and Philosophical Society—Some Account of the Lectures, with Suggestions. 8vo. 1882.
Ouvry, Rev. Peter Thomas, M.A. M.R.I. (*the Author*)—Practical Sermons. 16mo. 1882.
Pharmaceutical Society of Great Britain—Journal, March, 1882. 8vo.
Photographic Society—Journal, New Series, Vol. VI. No. 6. 8vo. 1882.
Ramsay, A. Esq.—Scientific Roll, Part I. No. 6. 8vo. 1882.
Royal Society of London—Proceedings, No. 218. 8vo. 1882.
Society of Arts—Journal, March, 1882. 8vo.
Sprengel, H. Esq. F.R.S. (*the Author*)—Sprengel's Vacuum Pump (commonly called Bunsen's Pump). 8vo. 1881–2.
Statham, H. H. Esq. (*the Author*)—Notes on Ornament. (Lectures delivered at the Royal Institution.) (Portfolio, March, 1882.)
St. Pétersbourg, Académie des Sciences—Bulletins, Tome XXVIII. No. 1. 4to. 1882.
Mémoires, Tome XXVIII., Nos. 8, 9. Tome XXIX. No. 1. 4to. 1881.
St. Petersburg Central Physical Observatory (*through Dr. H. Wild, Director*)—Annalen, 1880. 4to. 1881.
Symons, G. J.—Monthly Meteorological Magazine, March, 1882. 8vo.
Telegraph Engineers, Society of—Vol. XI. No. 40. 8vo. 1882.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1882: No. 2. 4to.
Vernon-Harcourt, L. F. Esq. M.A. (*the Author*)—Treatise on Rivers and Canals. 2 vols. 8vo. 1882.
Victoria Institute—Journal, No. 60. 8vo. 1882.
Wild, Dr. H.—Repertorium für Meteorologie. Band VII. Heft 2. 4to. 1881.

WEEKLY EVENING MEETING,

Friday, April 21, 1882.

WARREN DE LA RUE, Esq. M.A. D.C.L. F.R.S. Vice-President, in the Chair.

JAMES DEWAR, Esq. M.A. F.R.S. *M.R.I.*

Fullerian Professor of Chemistry at the Royal Institution, and Jacksonian Professor in the University of Cambridge.

Experimental Researches of Henri Ste. Claire Deville, Hon. M.R.I.

(Abstract deferred.)

WEEKLY EVENING MEETING,

Friday, April 28, 1882.

Sir Frederick Bramwell, F.R.S. Vice-President, in the Chair.

F. A. Abel, C.B. F.R.S.
President of the Institute of Chemistry.

Some of the Dangerous Properties of Dusts.

When dealing with the subject of so-called accidental explosions, in a discourse delivered to the Members of the Royal Institution, in March 1875, the lecturer pointed out that combustible, and especially inflammable substances, if sufficiently light and finely divided to allow of their remaining for some time suspended in air in considerable quantity, so as to form an intimate mixture with it, may, when ignited in this condition, produce explosive effects. The combustion of the finely divided particles which, under such conditions, are first inflamed, at once communicate flame to those in their immediate vicinity, and combustion is thus transmitted by and through the surrounding mixture of dust and air with a rapidity regulated by the inflammability of the dust, and by the proportion and state of division in which it is distributed through the air. If a rapidly burning mixture of this kind is confined, its combustion will be attended by explosive effects, the degree of violence of which is determined by the combustibility of the dust, by the quantity of mixture ignited, and the nature of its confinement. Its behaviour is indeed quite similar to that of a mixture of inflammable gas or vapour and of air; at the instant of its ignition each dust-particle is to a more or less considerable extent converted into inflammable vapour, or is, at any rate, surrounded by an envelope of burning vapour, so that if the particles are in sufficiently close proximity to each other, the rapidly successive development of vapour from them as the flame spreads, gives rise to a condition of things very like that which obtains when an inflammable gas, or vapour, originally existing as such, is mixed with air.

Even the most inflammable solid, in the form of dust, must be mixed in large proportion with air, must, indeed, be present in the form of a dense cloud, in order that the transmission of flame may proceed continuously from the portion first ignited to surrounding parts of the mixture. A dense cloud of Lycopodium dust in air will transmit flame with rapidity and violence throughout its whole extent, but if the particles in the cloud be not in very close proximity, the application of flame to it will only produce short flashes in the

vicinity of the source of flame, and the fire will not spread to surrounding particles.

The difficulty of maintaining, if only for a brief period, a sufficiently uniform and highly charged mixture of air with even a very light inflammable powder, to ensure the propagation of flame through it, and the circumstance that, with powders which are not very highly and completely inflammable, only some portion of the combustible matter is actually burned when flame is applied to the mixture of dust and air, necessitate the presence of a proportion of dust more or less considerably exceeding that which is proportionate to the oxygen supply in the volume of air with which it is mixed, if flame is to be transmitted by the mixture.

This condition is not difficult of fulfilment in practical operations in which inflammable dust is dealt with, and flame may consequently be transmitted upon a large scale through mixtures of inflammable dusts and air, with a rapidity calculated to produce more or less violently explosive effects, as has been demonstrated by many accidents in works where manufacturing operations have been attended by the production and escape into the air of large quantities of inflammable dust. The accidental inflammation of sulphur dust in chambers in which its pulverisation has been carried on, has given rise to more than one considerable and somewhat violent explosion. Cotton mills have been known to become rapidly fired by the ignition of, and transmission of flame by, mixtures of cotton dust and air, and very quickly spreading conflagrations originating from dust-explosions have occurred in other works dealing with even less inflammable and dust-producing materials; thus at the Guarancine Mills, at Sorgues, an explosion occurred in 1878, consequent upon the ignition of a mixture of air with the dust of that substance. But the most numerous and extensive calamities connected with the accidental ignition of mixtures of light inflammable dust and air have occurred in flour- and rice-mills.

The cause of many disastrous explosions and fires which occurred in flour mills at Budapest in Hungary, at Frideat in Germany, in other parts of the Continent, and in England, prior to 1872, appeared enveloped in much mystery, until Dr. Watson Smith directed attention to the fact that an Austrian observer had apparently traced their origin to the ignition, by flame or some incandescent body (such as sparks produced by the millstones), of mixtures of air and the dust of meal and husks formed during the grinding of corn or subsequent treatment of flour. The occurrence of a very serious explosion and fire at the Tradeston Flour Mills in Glasgow, in January 1872, caused that gentleman to direct public attention to what appeared the true explanation of these disasters, and on the occasion of that catastrophe, when several persons were killed and a number injured, the subject was carefully investigated by Messrs. Rankin and Macadam. The origin of the explosion was conclusively traced to the striking of fire by a pair of millstones, through the stopping of the feed, and the

consequent friction of their bare surfaces against each other; the results being, the ignition of the mixture of air and fine flour-dust by which the millstones were surrounded, and the rapid communication of flame thereby to the mixture of dust and air which filled the conduits in communication with the exhaust box: this being the common receptacle into which the mixture of dust and air is drawn, by an exhaust fan, through the conduits communicating with the several mills. From the exhaust box, where a portion of the suspended flour-dust was deposited, the air, still laden with dust, passed, in the Tradeston as in other flour mills, to another chamber, called the stive room, where a further quantity of the flour dust would deposit. A connected series of channels and larger enclosed spaces was therefore filled with a dust-laden atmosphere, through which flame was so rapidly transmitted from the millstones where the first ignition occurred as to produce violent explosive effects, which succeeded each other with very great rapidity in different parts of the building. The production of the blaze at the millstones was observed to be immediately succeeded by a crackling noise as the flame rapidly spread through the conduits to the exhaust box upon an upper floor, whence a loud report almost at once proceeded.

Messrs. Rankin and Macadam's inquiries elicited the facts that other flour-mill explosions had been attended by a similar succession of effects to those above indicated, and that at the Tradeston Mills themselves a less violent explosion, resulting in the bursting open of an exhaust box, attended by injury to some workmen, and the blowing out of windows and loosening of tiles, had taken place on a previous occasion. In the later accident, the more violent explosion of the exhaust box was followed by other distinct explosions in distant parts of those extensive mills, to which fire was led by the dust-laden air existing in the many channels of communication, and in which the cleansing and sifting operations, all attended by the escape of dust, were carried on.

Messrs. Rankin and Macadam ascertained that accidents of this nature at flour mills were of frequent occurrence, especially since the exhaust arrangements had been applied to the larger flour mills, and in their report they point out that it seems scarcely possible to guard against such accidents, though their frequency may be reduced by adopting efficient precautions for avoiding the stoppage of the feed to the millstones and the access of nails or other iron particles to the stones; and by prohibiting the employment of naked lights in the vicinity of the mills or dust passages. They also suggest that measures should be taken to reduce, as far as possible, the violence of explosions and the risk of injury to life and property, by constructing all receptacles into which the dust-laden air is drawn or passed from the mills, &c., as lightly as possible, so as to offer little resistance to the sudden expansion due to the ignition of an inflammable mixture, and by placing such receptacles as the exhaust box and stive room outside the building.

Since the publication of Messrs. Rankin and Macadam's valuable

report, the accidents at flour mills appear, however, to have been scarcely less numerous or disastrous than before the date of the Tradeston catastrophe. Thus, in September 1874, a similar though less serious explosion occurred at the Port Dundas City Mills, and in May 1878, another flour-mill explosion, quite unparalleled for its destructive effects, occurred at Minneapolis, Minnesota, where eighteen lives were lost and six distinct corn mills were destroyed. Mr. Peckham, writing after the event from the University at Minneapolis, states that two dull explosions rapidly succeeding each other were heard by him, and on looking towards the manufacturing part of the city a large volume of black smoke was seen to envelop the spot where the Washburn A Mill stood, a column of smoke being at the same time projected to a height of several hundred feet. A storm was blowing at the time in the direction from the Washburn Mill to other mills in the neighbourhood, and in about five minutes from the time that the explosion was heard five neighbouring mills, with adjoining premises, were in flames. Persons who were in close vicinity to the scene of the calamity at the time of the first explosion heard a succession of sharp hissing sounds, doubtless caused by the very rapid spread of flame through the dust-laden air in the passages leading from the mills to the exhaust box, and, at the instant of the explosion, the Washburn A Mill was observed to be brilliantly illuminated from top to bottom. The nearest mill to the latter was 25 feet distant, and appears to have exploded directly the flames burst through the first mill. The explosion of a third, 25 feet distant from the second, followed almost immediately; and the other three mills, about 150 feet distant in another direction, were at once fired. Windows were thrown out of buildings about a quarter of a mile distant, consequent upon the back rush of air following the explosion, and portions of the building materials were projected to very considerable distances. The cause of the explosion was carefully inquired into (by Messrs. Pick, Peckham, &c.), and it was attributed to fire being generated by the stoppage of the feed to a pair of stones, or by the accidental passage of some very hard substance between them. The consequent explosion of dust-and-air mixture round the stones and in the communicating passages added, by its concussion, to the quantity of dust suspended in the air in different parts of the mill, and a second more violent explosion was thus immediately brought about. The attention of Professor Lawrence Smith was directed to the subject of flour-mill explosions by this accident, and, in a letter to M. Dumas, of May 4th, 1878, which was published in the 'Annales de Chimie et Physique,' he states his conviction, based upon experimental inquiry, that such accidents are due to the formation of explosive mixtures of finely divided organic matters (such as flour) with the air, and refers to this as a "revelation" of the existence of a previously unknown danger connected with an important industry, being apparently unaware of its elucidation by Rankin and Macadam, and Watson Smith in 1872.

Attention has again been recently directed to this subject of flour-dust explosions by a fatal and extensive calamity of the kind which occurred at a flour mill at Macclesfield in September 1881, and has been made the subject of an interesting report to the Home Secretary by Mr. T. J. Richards, of the Board of Trade, in which he confirms the conclusions of Messrs. Rankin and Macadam, and repeats the recommendations made by them.

In this particular case, again, there appears to have been no doubt that the inflammation of the dust-and-air mixture surrounding a particular pair of millstones was due to the stones remaining empty for some time, sufficient heat being consequently developed to ignite some portions of flour dust existing between the bearing surfaces. One of the owners of this mill deposed that he had seen flame produced by stones when remaining empty, and that the appearance of the stones in question convinced him that flame had been thus produced. A very dry grain was, moreover, being ground at the time of the explosion. A strong consensus of opinion appears to exist that it is very difficult, with the best arrangements for feeding the mill-stones with grain, to guard against their running empty occasionally, and there is no doubt that on these occasions portions of flour are exposed to heat sufficiently great to char and sometimes even to ignite them. In connection with this effect of the heat to which portions of flour may be exposed between "dry" stones, the opinion of an "experienced person" (quoted as a regrettable one by Mr. Richards) deserves not to be lost sight of. It is to the effect that a stive room can at all times be safely entered with a naked light "except when there is observed the peculiar odour which is noticed there when one of the millstones has been previously running empty." It is not difficult to demonstrate that fine flour *very* thickly suspended in air will produce with the latter an inflammable mixture, through which flame will be rapidly transmitted; there is also no doubt that if, as is frequently the case, the enclosed dust-and-air mixture in the air-passages of a mill is somewhat warm, the propagation of flame through the mixture will be facilitated. But experimental observations which the lecturer has had occasion to make in connection with another branch of the subject of this discourse, lead him to consider it not impossible that the development of even very small quantities of inflammable gas or vapour from flour particles which become heated between "dry" stones to an extent to be charred, may, in some cases, decidedly facilitate the propagation of flame by a particular mixture of dust and air, which might otherwise only be bordering upon an explosive mixture.

Mr. Richards calls attention, in an appendix to his report, to four very disastrous fires which had occurred in flour mills at Wakefield, York, Liverpool, and Deptford, within two months of the completion of his report, the origin of the fire being in each case unknown. There is no doubt that the number of fires occurring in corn- and rice-mills, the origin of which is wrapped in obscurity, is very great;

and it is stated upon good authority that only about 20 per cent. of the explosions in flour mills which can be actually substantiated, are made public, the miller being unwilling to direct increased attention to the risks of his business, which, as it is, have given rise to the establishment of high rates of insurance upon corn mills. If efficient measures can be adopted in mills for preventing the dispersion of fine flour-dust by other than the comparatively imperfect contrivances for promoting its partial deposition (as in the exhaust box and stive room), flour-mill explosions will certainly be reduced in frequency and importance. The efficiency of at any rate one simple device for arresting the dust, by a species of filtration of the air which is removed from the millstone chambers, seems to have been already decisively demonstrated by practical results, and there appears reason to hope that the millowner will ere long have no valid excuse for permitting a continuance of conditions favourable to what have appeared to be hidden risks of danger to his property and to the lives of those whom he employs.

There appears no doubt that some instances of explosion or of very rapidly spreading fire in flour mills have been ascribable to the employment—accidentally, or with the permission of those in authority—of naked lights in the vicinity of particular parts of the factory where dust may be thrown into the air in large quantities. An explosion from this cause occurred at the mills of Messrs. Ellis and Co., of Bradford. A spout from a sieve having become choked, a man removed the lid; a quantity of dust at once flew out, and the mixture, meeting either a lamp in the man's hand or a naked gas flame close by, exploded, rendering the man insensible; the flame passed along an enclosed belt to a box containing a fan which was driving a blast of air into five purifying chambers; these purifiers were fired simultaneously, and the explosion then passed to the adjacent exhaust purifiers and thence to the dust room, so that the mill was fired throughout almost immediately. In another instance, the floor of a meal chamber broke, letting through the flour, which, on falling into the air, was ignited by a flame in the vicinity, and speedily fired the mill. Judging from statements made at a recent meeting of the National Association of British and Irish Millers, the opinion is entertained by many millowners that the running of millstones empty must not be credited with too great a share in the origination of explosions or fires in mills; but that many are caused by the so-called accidental ignition (by naked flames) of dust-and-air mixtures. If such be the case, grave responsibilities are incurred by millowners and managers who permit the existence of lights other than safety lamps in localities where there is any possibility of a considerable quantity of dust becoming suspended in the air, or do not establish and strictly enforce regulations prohibiting the carrying of naked lights in or near any working part of the mill.

The important part played by coal dust, which exists in greater or less abundance in all coal-mine workings, in aggravating and extending

the injurious effects of fire-damp explosions, was originally pointed out with great force by Messrs. Faraday and Lyell, in the report which they submitted to the Home Secretary, in 1845, on the explosion at the Haswell Collieries in September 1844, and on the means of preventing similar accidents. It does not come within the scope of this discourse to examine into the chief part of this most interesting and instructive report, which deals exhaustively with the cause of the explosion and the means of guarding against the recurrence of such a calamity; but the lecturer, having had occasion to study carefully what has been published on the subject of coal-mine explosions and their causes within the last three years, cannot forbear pointing out that the observations and conclusions published by Faraday and Lyell thirty-seven years ago have been repeatedly re-clothed with the garb of originality by workers who have but extended and amplified the original observations of those eminent men.

After discussing the subject of the accumulation of fire-damp in the goaves of the mines, its dislodgement by the drawing of juds, by falls of the roofs in the goaves, and by changes in atmospheric pressure, its diffusion into the surrounding air in the mine ways, its ignition by a defective lamp, and the spreading of the flame to the gas-mixture with which the goaf was charged, the reporters say: "In considering the extent of the fire from the moment of the explosion, it is not to be supposed the fire-damp was its only fuel; the coal dust swept by the rush of wind and flame from the floor, roof, and walls of the works would instantly take fire and burn, if there were oxygen enough present in the air to support its combustion; and we found the dust adhering to the faces of the pillars, props, and walls in the direction of and on the side towards the explosion, increasing gradually to a certain distance as we neared the place of ignition. This deposit was in some parts half an inch, in others almost an inch thick; it adhered together in a friable coked state. When examined with the glass it presented the fused round form of burnt coal dust, and when examined chemically and compared with the coal itself reduced to powder, was found deprived of the greater portion of the bitumen, and in some instances entirely destitute of it. There is every reason to believe that much coal gas was made from this dust in the very air itself of the mine, by the flame of the fire-damp, which raised and swept it along, and much of the carbon of this dust remained unburnt only for want of air.

"At first we were greatly embarrassed by the circumstance of the large number of deaths from choke-damp, and in the evidence that *that* had been present in very considerable quantities compared with the small proportion of fire-damp, which, in the opinion of those in and about the works just before, must have occasioned the explosion. But, on consideration of the character of the goaves as reservoirs for gaseous fuel, *and the effect of dust in the mine*, we are satisfied that these circumstances fully account for the apparent discrepancy."

On January 17th, 1845, Faraday delivered a discourse to the

members of the Royal Institution, in which he dealt with the substance of the above report, and with the experimental inquiry made by himself with reference to the provision of means for preventing a recurrence of such disasters as that at Haswell. In a brief account of this lecture published in the number of the *Athenæum* following its delivery, the substance of his remarks relating to the effect of coal-dust is given in these words: "The ignition and explosion of the (fire-damp) mixture would raise and then kindle the coal dust which is always pervading the passages, and these effects must in a moment have made the part of the mine which was the scene of the calamity glow like a furnace."

The report of Faraday and Lyell was published in the 'Philosophical Magazine' for January 1845, and was followed by a letter from Faraday in the February number of the same publication, in which he referred to the lecture just delivered at the Royal Institution, and made further suggestions with respect to the method of ventilating the mines suggested in the report. But it appears that these publications remained long unknown in France, for in 1855 M. du Souich, Chief Government Mining Engineer of the Saint Étienne arrondissement, when referring to an explosion which had occurred at Firminy, advanced, as new, the view that the deposition of crusts of a light coke upon the props was due to dust which was swept up and transported to a distance by the violent current produced by the explosion, and which, being in part inflamed, would carry on and prolong the effects of the fire-damp. The fact that men near the pit's mouth received burns and other injuries, while others who were in workings near the seat of the explosion, but out of the main air-current, escaped unhurt, was ascribed by him to this ignition and carriage of flame by dust. Had the results of the explosion been entirely due to the mine being highly charged with gas, the explosion must, he considered, have extended to those portions. On the occasion of two explosions in 1861, M. du Souich again dwelt upon his views regarding the part played by coal-dust in increasing the disastrous effects of fire-damp explosions. In 1864–67, M. Verpilleux instituted experiments which led him to the conclusion that coal dust plays an important part in coal-mine explosions; the subject was also pursued by several other French mining engineers at about the same time, and especially by M. Vital, who made some experiments on a small scale, in 1875, in connection with an inquiry into the nature and cause of an explosion which had occurred the year before at the Campagnac Colliery, and in a part where no fire-damp had ever been detected. An examination for gas had been made by the overman with a Mueseler lamp just before a shot was fired, and after the first shot, a second shot was prepared, and the fuze having been ignited, the men retreated, when, after a short interval, an explosion took place, and the men stated that they saw a body of reddish flame advancing upon them. After examining the nature of dust collected in the mine, and instituting some special experiments upon a very

small scale for the purpose of ascertaining whether, and to what extent, the flame from a small charge of powder was lengthened, when projected, like the flame from a blown-out shot, into air containing fine coal dust in suspension, M. Vital concluded that very fine coal dust, very rich in volatile (inflammable) constituents, will take fire when raised by an explosion, and that portions of the coal are successively decomposed, yielding explosive mixtures with the air, whereby the fire is carried along; the intensity or violence of the burning being much influenced by the physical characters (fineness, &c.) of the dust. He also pointed out that an explosion of fire-damp, while taking place almost instantaneously, inflames or decomposes a small quantity of coal dust raised thereby; explosive action being thus propagated after the fire-damp explosion has ceased. Soon after M. Vital's investigation of the subject, Mr. W. Galloway commenced a series of valuable experiments upon a larger scale, with the view of investigating the influence of coal dust in colliery explosions, and the results were communicated by him to the Royal Society in two papers in 1876 and 1879. The conclusions to which Mr. Galloway was led by the experiments described in his first paper, were to the effect that a mixture of air and a particular coal dust which had been made the subject of chemical examination and practical experiment was not inflammable at the ordinary pressure and temperature, but that the presence of a very small proportion of fire-damp in the air, the existence of which could not be detected with the Davy lamp by the most experienced observer, rendered this dust inflammable, and caused it to burn freely with a red, smoky flame. From this it was inferred that an explosion, when originated in any way whatever in a dry and dusty mine, may extend itself to remote parts of the workings, where the presence of fire-damp was quite unsuspected.

In his second paper, Mr. Galloway shows that the return air of a fiery mine which, though furnishing no indication of the presence of gas when examined in the usual way (by means of a Davy safety lamp), might in his opinion contain from 2 to 2·5 per cent., may be rendered inflammable by suspending coal dust in it. He also described experiments by which it appeared to be demonstrated that the flame produced by the explosion of fire-damp in a particular part of a mine might be propagated, at any rate to some extent, by coal-dust raised by the explosion and suspended in the air travelling through the mine, even in the *complete absence* of fire-damp in the air. The apparatus used by Mr. Galloway was constructed on a somewhat extensive scale. In connection with the channel or gallery through which a current of air, with or without coal dust in suspension was passed, was a receptacle in which a mixture of pit gas (from Llwynpia Colliery) and of air was prepared and exploded. The direct communication between the gas vessel and the gallery (representing a mine way) was only interrupted by a diaphragm composed of from two to six leaves of newspaper; this separator being burst through by the explosion of a mixture of nearly two cubic feet of fire-damp with the requisite pro-

portion of air. The coal dust was placed on the floor of the gallery and upon certain shelves fixed in it. It appeared open to question whether, with the employment of this apparatus, there was not a possibility of very small quantities of fire-damp penetrating, before the explosion, into the gallery from the explosive chamber, through the closing arrangement above alluded to, and whether the results obtained in the gallery might, consequently, be accepted as produced solely by the effect of the concussion produced and flame promoted by the gas explosion in the separate chamber.

In a paper just communicated to the Royal Society, Mr. Galloway argues that any amount of gas which may thus escape into the gallery must be altogether insignificant as regards any possible influence upon the results obtained.

The conclusion now arrived at by Mr. Galloway, as the result of continued experiments with this apparatus, of which he has just given a further account, and of his examination into the effects produced by the Penygraig explosion in December 1880, and the Risca and Seaham explosions of that year, is confirmatory of that published by him last year, namely, that the very decided view which he first held, "that a mixture of air and coal-dust is not inflammable at ordinary pressure and temperature without the presence of a small proportion of fire-damp," has not been borne out by his further experiments, as he considers that he has now shown "conclusively that fire-damp is altogether unnecessary for the propagation of flame with explosive effects by a mixture of coal dust and air," when the scale on which the experiments are made is large enough, and when the fineness and dryness of the dust are "unquestionable."

This conclusion coincides in the main with that arrived at in 1878, as the result of experiments by Professor Freire Marreco, conducted in connection with the North of England Institute of Mining and Mechanical Engineers, which Society, as well as the Chesterfield and Derbyshire Institute of Engineers, has laboured very usefully in this direction cotemporaneously with Mr. Galloway. The most recent conclusions of the latter in respect to coal dust were in fact forestalled by those which the late lamented Professor Marreco in association with Mr. P. D. Morison communicated to the first-named Institute in November 1878, and which were published in its Transactions of that date.

Messrs. Marreco and Morison's experiments were carried out in galleries or long boxes, representing mine workings, though on a smaller scale than Mr. Galloway's later apparatus, and constructed somewhat differently in their details. The apparatus used by them at Harton Colliery (and with which experiments have since been continued by Messrs. Lindsay Wood and G. May), was in fact a double gallery, so arranged that the air current which passed into one gallery made its exit at the end of the second, alongside the point of its first entrance. The mode of proceeding was to fire successively two powder shots, in different positions in the gallery box,

from small cannon, so as to represent blown-out shots in the effects produced; coal dust was placed upon the floor of the box, and one shot was first fired against the air current which was passing at a known velocity. The dust cloud thereby raised was carried along by the current and a second shot was fired into it, and, in a large number of experiments made with many different descriptions of dust, the flame produced by the second shot was increased by that of inflamed dust, a comparatively clear flame being sometimes produced, while in other instances it was accompanied by a shower of sparks. The view taken by Vital, Marreco, and others, regarding the action of coal-dust in propagating flame in air free from fire-damp, is to the effect that the first portions of dust acted upon by the inflamed gases of the shot, liberate inflammable gas which mixes with the air, and is fired, the non-volatile part of the coal being in part consumed and in part deposited as a feeble coke. Some examination of coked deposits of dust sent to Marreco subsequently by Mr. Galloway, confirmed the observations originally made by Faraday and Lyell, that the coal dust is in part submitted to destructive distillation during the progress of the flame through the dust-laden air. Marreco considers that, although a proportion of the heat developed by the burning dust is absorbed by the gasification of the coal-constituents, the heat of combustion of these suffices to leave a margin for the carrying on of the action from one particle of dust to another, provided these be in sufficiently close proximity to each other.

In the experiments made by the Chesterfield and Derbyshire Institute of Engineers, in a very long gallery, results were obtained very similar to those of Marreco and Morison, and it was also found that a lengthening of a gas flame, which was placed in the gallery, could be obtained by causing the current of air to carry with it thick clouds of some descriptions of coal dust.

Many instances are on record in this country and others of the firing, with semi-explosive violence, of clouds of coal dust, produced either in the open air, or in localities where no fire-damp could exist, some portions of the mixture of dust and air having come into contact with a flame or fire. Thus Marreco and Morison mention a case of a considerable quantity of coal dust, which had been accidentally thrown over some screens at a pit's mouth, flashing into flame as the dust cloud came into contact with a neighbouring fire, and burning a man very severely; and another accident, which occurred in a stone-drift, where it was believed that no gas could possibly be present. A considerable body of rock was dislodged and coal dust raised by the firing of a shot, the flame of which fired the air-and-dust mixture, with very mischievous results. From 50,000 to 60,000 cubic feet of fresh air were said to be passing through the drift per minute when this accident occurred.

There appear good grounds for believing that, provided coal dust be sufficiently fine and thickly suspended in the air, and of a readily inflammable nature, fire may travel to a considerable distance in the

working of a mine, through its agency, in the complete absence of fire-damp. The effects of transmission of flame in this way would be decidedly different, and much inferior in violence, to those produced by an explosion of fire-damp and air, or of a mixture of these with coal dust; the comparative suddenness of the gas explosion would produce greater destruction and less burning effects than the comparatively gradual explosion, or the rapid burning of a dust-and-air-mixture. In the latter case, the coal dust will generally be considerably in excess of the air needed for its combustion, so that, however finely divided, much will escape being burned, and may be only very partially coked, and it is conceivable that, as suggested by Mr. Galloway, a second rapid burning or semi-explosion may be caused by the inrush of air, following the first explosion, into the workings, which may be thick with heated and only partially burned dust, some of which may still be incandescent.

Considering that, since first Faraday and Lyell directed attention to the dangers of coal dust in mines, its behaviour has been made the subject of many series of experiments and published reports here and abroad, it is remarkable that in most instances of coal-mine explosions, until quite recently, the probable effect of coal dust in increasing their magnitude does not appear to have received the serious attention which it merits at the hands of mine owners and of those in authority connected with coal mines. When the Royal Commission on Accidents in Mines was appointed, it collected evidence from H.M. inspectors of mines, from experienced colliery owners and mining engineers, and from selected pitmen, with respect to the causes of accidents, and that evidence included several statements regarding the possible influence of coal dust in aggravating explosions, but the preponderance of opinion of H.M. inspectors was against the view that explosions could originate with, or be to any great extent propagated by coal dust *in the absence* of fire-damp. The only experiment on a practical scale bearing upon the subject which appears to have been made until quite recently is that of Mr. H. Hall, Mine Inspector of the N. Wales, &c., District, who, in firing charges of 4 lb. of powder from a cannon in an adit driven about 50 yards from the surface in a coal seam on the dip, coal dust being sprinkled upon the floor, obtained flame extending to distances of 30 to 60 yards, while without the dust the flame of the shot did not extend more than 6 or 7 yards.* Some decided opinions were expressed that the supposed influence of coal dust in aggravating explosions was over-rated, and that it would certainly not lead to explosions in the absence of gas. On the other hand, Mr. Galloway expressed a strong opinion that some of the most extensive of recent explosions, such as those at Llan and Abercarne, were at any rate largely contributed to by coal dust, and more recently, on the occasion of the inquiry into the Penygraig explosion, he gave evidence

* Mr. Hall stated that the air in this adit was "practically" free from gas, but did not maintain its *absolute* freedom.

to the effect that the disastrous results of this explosion were mainly if not entirely ascribable to the action of coal dust, supporting this opinion by the results of a minute examination into the condition of the pit, of the sufferers, &c., after the accident.

When the terrible calamity which occurred at Seaham Colliery in September 1880, was officially inquired into, the suggestion was very decidedly put forward by the miners' representatives, that the coal dust which existed in large quantities in some parts of the mine, and especially near the spot where it was surmised that the explosion had originated, might have had much to do with the accident. Indeed the opinion was strongly entertained by some that it was entirely due to the ignition of coal dust, in the absence of gas, by the flame from a blown-out shot. The lecturer was consequently requested by the Home Secretary to make experiments with samples of dust collected in different parts of the mine, and the results obtained with them led to an extension of experiments with dust from other collieries in different parts of the kingdom. These experiments, carried to a certain point for the immediate purpose of the Seaham inquiry, have been interrupted for some time, but the Royal Commission has now resumed them with the object of obtaining more precise data in connection with certain results which were elicited by the first part of the investigation.

The earlier experiments were carried on at the Garswood Hall Colliery, where a constant and abundant supply of pit gas (a so-called blower) is brought to the surface, and was kindly placed at the service of the Commission by Messrs. Smethurst and Co., together with many conveniences, for the purposes of these and other important experiments upon which they have been engaged. The apparatus used at Garswood for the experiments with the Seaham and other dusts, was similar in character to those employed by Freire Marreco, Galloway, and others, great pains being taken to secure accuracy and uniformity in the velocity of the air currents passing through the gallery, in the proportion of pit-gas, or fire-damp, used with the air, and in the intimacy of the mixture. In order to raise the air current in the gallery to a temperature similar to that of the atmosphere in colliery workings, the air supply was drawn through a system of heated pipes, so that, when passing at as high a velocity as 1000 feet per minute, its temperature would be raised up to 80° or 85° F. even in the very severe weather during which some of these experiments were made.

The samples of coal dust experimented with were examined with respect to fineness, proportions of volatile matter and ash, and one or two other points, and they were all carefully dried before use.

Experiments were made in the first instance with a view of ascertaining the smallest proportion of fire-damp which, when mixed with the air passing through the apparatus, would furnish an atmosphere capable of firing at a naked flame of a particular size, placed in the gallery. It was next ascertained what quantity of gas below that proportion was needed to impart to the mixture of air with

a large quantity of each particular coal dust the property of exploding throughout the gallery. By these experiments the samples were classed in the order of their sensitiveness to explosion, and it was found that those which were very rich in pure coal, and which contained the highest proportion of very fine dust were the most sensitive, i.e. required the lowest proportions of fire-damp in air to bring them to explode readily when suspended in a dense cloud. But with the samples containing larger proportions of non-combustible matter the order of sensitiveness did not necessarily harmonise with the comparative richness of a sample in pure coal, nor with its comparative fineness, and this was strikingly illustrated by a sample of dust from one of the roads in Seaham Colliery, which contained more than half its weight of non-combustible matter, yet ranked only third in order of sensitiveness, while another sample, containing considerably more coal and a somewhat larger proportion of the finer dust, ranked fifth.

Another point clearly established, and confirming by more accurate data the observations of earlier experiments, was, that the proportion of fire-damp required in a mine to bring dust into operation as a readily exploding material when thickly suspended in the air is bordering upon and even below the smallest amount which can be detected in the atmosphere of a mine, by the most practised observer, with the use of the Davy lamp, the only means of searching for gas which has until quite recently been employed in mines. The highest proportion which can be thus detected by an experienced operator is stated to be about 2 per cent. Explosions were produced by dusts suspended in air travelling at a velocity of 600 feet per minute, when fire-damp was present in proportions ranging from to 2 to 2·75 per cent.; in currents of low velocity the same result was produced with a sensitive dust in the presence of only 1·5 per cent. of fire-damp, and ignitions which approached explosions in their nature and extended to considerable distances, were obtained with this dust in air containing still smaller proportions of gas. Mixtures of fire-damp and air bordering upon those which will ignite upon the approach of flame, were found to be instantaneously fired by a lamp if they contained only a few particles of dust in suspension, and in connection with this fact the interesting observation was made that such dust particles need not be inflammable nor combustible to produce the result named. Mixtures of air and gas which passed a naked flame without any symptom of ignition, were inflamed when particles of a fine light powder, such as calcined magnesia were suspended in them. The action of certain of the pit dusts which contain comparatively little coal, in determining the ignition of mixtures of air and small proportions of fire-damp, is possibly of the same character as the behaviour of such a dust as calcined magnesia. The power of favouring the ignition of mixtures of fire-damp and air was not exhibited by some other powders similar in fineness to the latter, but differing in structure and density from this and one or two other non-combustible dusts which may be called active;

even different samples of magnesia, differing somewhat in lightness from each other, appeared to possess the activity in different degrees. These facts seem to favour the view that a dust possessing particular physical characteristics exerts a contact- or catalytic action upon gas mixtures, similar to that known to be possessed by platinum and some other substances under particular conditions. Thus, when finely divided platinum, or even a clean recently heated surface of the compact metal is brought into contact with mixtures of hydrogen, or of a hydrocarbon gas or vapour, with oxygen or air, oxidation of the hydrogen or hydrocarbon is at once established, accompanied by the development of heat, whereby the temperature of the metal is raised and chemical activity promoted, so that heat speedily accumulates, raising the metal to a temperature sufficiently high to bring the surrounding gas-mixture to the exploding point. If the metal presents a very large surface, or is in a specially porous condition, as in the form of sponge or very fine powder (*platinum black*), the explosion of the gas-mixture may follow very rapidly, or almost instantly, upon the first contact of some portion with it.*

In many of the experiments with calcined magnesia just referred to, it was distinctly noticed that a dark space intervened between the gas flame used as the source of heat and the flare produced by the ignition of the gas mixture through the influence of the dust cloud suspended in it, which would seem to indicate that the dust particles, immediately upon passing through the flame, established some amount of oxidation of the fire-damp, which proceeded with increased rapidity as the dust became more highly heated through the chemical action developed, so that within a short distance from the point where the heating commenced the dust became incandescent, and the ignition of the gas-mixture followed. Further experiments which are contemplated may elucidate the precise nature of this action of non-combustible dust in promoting the ignition of gas-mixtures which, in the absence of dust, are not inflammable; there appears little doubt, however, that it constitutes one element in the dangers arising from the presence of dust in the air of a mine which contains a small proportion of fire-damp, and in which a large body of flame is accidentally produced, either by a blown-out shot, or by a fire-damp explosion of local character.

Numerous experiments similar to those of Marreco and Morison were made by the lecturer at Wigan with mixtures of air and coal-dust from Seaham and other collieries, in the complete absence of

---

* This action of platinum (or palladium) has recently received applications bearing special reference to the existence of explosive gas mixtures in coal mines. The one consists in an apparatus proposed by Mr. Körner for removing, by slow combustion, local accumulations of fire-damp; the other is a very simple and portable photometric apparatus, devised by Mr. G. H. Liveing, by which proportions of fire-damp much lower than the smallest amount discoverable by the Davy lamp in the hands of the most expert, can be readily and quickly detected, and the amount estimated with considerable accuracy.

fire-damp, which were passed through the apparatus at different velocities up to 1000 feet per minute. Small cannon, specially constructed to ensure uniformity in the volume of flame produced at different times, were fired in them, either singly or in pairs in rapid succession; and exposed heaps of guncotton and of slow- and quick-burning gunpowder were exploded in the dust-laden air. The results occasionally confirmed to some extent those of Marreco and Morison and the Chesterfield experiments. At velocities of 400 feet per minute the dust, which was either passing at the time or was raised by the concussion of a first shot, did not appear to produce any increase in the volume of flame furnished by the cannon, but a decided though inconsiderable lengthening of the flame was several times observed at higher velocities and with the employment of the most inflammable dusts. Some of these, when thickly suspended in air travelling at velocities of 500 to 1000 feet per minute, and exposed to the action of a large flash of flame (as produced by the loose heaps of guncotton and *blasting* powder), exhibited a tendency not only to burn explosively in and close around the flame, but also to propagate flame, or cause it to travel along some distance; but the most decisive results of these experiments were not of a nature to warrant the conclusion that flame could be carried along indefinitely, or even to a very considerable distance, by coal dust in the complete absence of fire-damp, as now maintained by Mr. Galloway. There can be no question that the scale of magnitude upon which the first ignition in the dust-laden atmosphere is produced must greatly influence the extent to which the propagation of flame in this way will extend, and Mr. Galloway's experiments at Llwynpia, therefore, were likely to develop conditions more nearly approaching those of the real state of things in a mine than experiments in galleries of smaller dimensions, and with small initiating volumes of flame. But the necessity for caution in deducting very decided conclusions from even large-scale experiments, appears to be illustrated by some of Mr. Galloway's results, inasmuch as some of the great distances to which the flame extended were observed under conditions decidedly favourable to the projection of the flame by causes which would not come into play in the same way in a mine-working. The experiments made some years ago by Mr. Hall in an adit (which have already been referred to) appear to have a more direct bearing upon results likely to be actually produced underground in a dust-laden atmosphere. In those experiments, the extreme distance to which flame was carried by dust, first ignited by the flame from a very excessive charge of powder (4 lb.), was 180 feet. It is of course possible that the coal used was not of the most inflammable description, and that its fineness and density were not most favourable to its becoming very thickly suspended in air. On the other hand, Mr. Hall stated, in his evidence before the Royal Commission, that the atmosphere in the adit was only "practically" free from gas.

The volume of flame from a blown-out shot in a mine-working is

generally considerable, but it appears that exaggerated estimates are entertained of the distance to which, *in the absence of dust*, the flame will be projected, and it is probable that the large volumes of flame, extending occasionally to many yards from the spot where the shot was fired, are in a great measure due to the ignition of dust raised by the concussion and rush of air at the instant of firing. Mr. Hall, in his experiments in the adit, found that the flame from the shot of 4 lb. of powder reached to a distance of only 18 to 21 feet when no dust was present. A few months ago that official directed the attention of the lecturer to the occurrence of two accidents in the Liverpool district, each one occasioned by a shot of 1 lb. of powder blowing out its stemming without shaking or bringing down any coal. In both instances the shot lighter and two pitmen had retired about 100 feet from the seat of the shot, that is, about 30 feet in a straight line with it, and 60 to 80 feet along both directions of a working running at right angles to the drift in the face of which the charge was fired. In the case of one accident, a man was killed, and serious injuries were sustained by the other men in both instances. There were signs of charring upon the props up to, and 5 or 6 feet beyond, where the men were standing, but they did not extend farther. The drift and the level in which these accidents occurred were 5 feet high and 12 feet wide. Mr. Hall informed the lecturer that a strong impression existed among mining men on the spot that the flame of the shot, quite unaided by gas or coal-dust (the latter was known to be present), would have extended so as to produce the effects described. This appeared so at variance with Mr. Hall's experiments in an underground working, and with Mr. Abel's own experience in other directions, that the latter has endeavoured to obtain some precise experimental data with regard to the distance to which any burning effect from a blown-out charge of 1 lb. or 1½ lb. of powder would extend in a mine-working, in the absence of dust. With this object he availed himself of the friendly assistance of Major Durnford, R.E., Instructor in Field Fortifications at the School of Military Engineering, Chatham, under whose direction Lieutenant Raban has carried out an instructive series of experiments in accordance with suggestions made by Mr. Abel as the work proceeded.

The locality selected for the first experiments formed a portion of some obsolete fortifications at Chatham, and consisted of a masonry gallery or *Caponier*, 8 feet 8 inches high to the spring of the arch, and 8 feet wide below the arch, to a distance of 28 feet from the closed end; from that point it tapered on one side to 6 feet along a length of 2 feet 6 inches, and was 6 feet wide for a length of 3 feet 6 inches, up to a pier or square column 4 feet by 3 feet 6 inches; round which the gallery curved, being at this part 4 feet 2 inches wide. The straight part of the gallery, from the dead wall at one end to the projecting pier at the other, was 34 feet long. In the wall to the left of the blocked end there were six narrow loop-holes up to the

curve, commencing at 18 feet from the end, and 2 feet 6 inches apart, in the opposite wall there were four, commencing at the same distance and 5 feet apart; over the wall at the blocked end of the gallery there was an opening into the outer air, and a considerable current of air passed through it along the gallery to the curved end, which led into a large narrow gallery at right angles to this wide one, and having large chambers opening into it.

In some preliminary experiments, an iron tube was let into the face of the wall at the blocked end of the gallery, so as to represent a strong blast hole, and this was charged with $1\frac{1}{2}$ lb. of powder, untamped in some experiments and tamped in others, some pieces of guncotton were suspended from the roof of the gallery at a distance of 28 feet and farther along, and observers were stationed outside the gallery opposite the several loop-holes. But, while the pieces of guncotton were not inflamed, there were conflicting opinions concerning the distances at which flame was seen, probably caused by the general illumination of the gallery by the flash of the explosion. It was, moreover, found that the iron tubes containing the charges were more or less considerably torn, so that portions of the exploding charge escaped laterally. The following method of experimenting was eventually adopted. Charges of $1\frac{1}{2}$ lb. and 2 lb. of powder, untamped and tamped, were fired from a small roughly bored out gun-block, the bore of which was 1 foot 9 inches long and $2\frac{3}{4}$ inches in diameter; the gun was raised so as to project the flame right along the gallery at about its centre. A light woodwork frame, 5 feet square, was fitted with thirty-six cross wires 1 foot apart, so as to furnish thirty-six points of intersection; to each of these points a small tuft of guncotton was attached, and the target thus fitted was fixed vertically so as to face the charge, in the centre of which was fixed an electric fuze. In this way small charges of guncotton were distributed uniformly over all parts of the target, which filled a great part of the section of the gallery. The distance of the target from the charge being gradually increased in successive experiments to 20 feet, it was found that with the employment of $1\frac{1}{2}$ lb. and 2 lb. charges, untamped, in three instances out of ten experiments only one, or at most two of the tufts of guncotton were inflamed, this being apparently the extreme distance to which flame, or matter sufficiently hot to inflame guncotton, was projected. At a distance of 19 feet, with $1\frac{1}{2}$ lb. charges, two out of three shots did not inflame any of the guncotton tufts. With $1\frac{1}{2}$ lb. charges firmly tamped, one tuft only of the thirty-six was fired, in two experiments, at a distance of 20 feet, while in three others no guncotton was inflamed.

It appears from these results that in a gallery or mine-working of an area not very dissimilar to that in which the accidents just referred to occurred, the flame or heated gases from $1\frac{1}{2}$ lb. and 2 lb. charges, fired under conditions favourable to the production of the maximum flame, and its complete projection in the direction of the discharge, only reaches occasionally, and to a very limited extent, to a distance of

20 feet. No doubt a powerful air current in a mine, passing in the direction in which the shot is fired, must have a tendency to aid the spread of the flame to a greater distance, but the difference between 20 feet and 100 feet, the flame having in the latter instance extended to a distance of 75 feet along a gallery at right angles to the point of ignition, is far too great to be only ascribable to the effect of an air current in elongating the flame. As the first of the loopholes above referred to existing in the walls of the gallery was 18 feet from the shot, they could hardly affect the distance to which the flame was found to reach.* It will be observed that these results correspond with those which Mr. Hall obtained with 4 lb. charges of powder in an adit, the dimensions of which are not specified.

No gallery of large dimensions and free from the small lateral openings was available for the continuance of these experiments, but it was thought that some experiments in subterraneous passages of much smaller dimensions (military countermines) might give instructive results. A so-called envelope gallery was therefore first selected for the purpose. This gallery was 5 feet 9 inches high to the crown of the arch, and 4 feet 9 inches to the springing of the arch, and only 2 feet wide. The part selected for the position of the gun and the target was straight, but the portion immediately beyond was curved. In rear of the gun, the gallery was quite open to a considerable distance. One-and-a-half pound charges, untamped, were fired, and a frame-target the width of the gallery and 4 feet 6 inches high, constructed so as to give 15 points for the attachment of guncotton tufts, was placed at gradually decreasing distances from the gun, commencing at 20 feet. Even at a distance of only 14 feet from the charge, none of the guncotton tufts were inflamed; but the target was blown forward about 12 feet and partly broken. It was evident that the fact of the gallery being open at the *rear* of the charge greatly reduced the tendency to the projection of flame to a distance in the direction of the explosion. The resistance opposed to the movement of the air by the curvature of this very narrow gallery, a short distance in front of the seat of the experiments, may have also contributed to diminish the distance to which the flame or highly heated gases would extend. When the experiments were continued in another gallery, of the same dimensions, but straight and terminating in a head, like a drift in a mine, the cannon being placed close up to the face of the drift, several of the tufts of guncotton were inflamed at a distance of 27 feet; one was inflamed when the target was 30 feet off, and one also at a distance of 32 feet, but none were ignited at a distance of 35 feet from the charges. Here then, in a long gallery, narrow in proportion to its height, but in all respects representing a drift way in a mine, the distance to which the flame of a blown-out shot of $1\frac{1}{2}$ lb. of powder extended was less than 35 feet, and therefore considerably less than one-half the distance from the seat of the blown-out shot of 1 lb. of

* The closing up of these was not found to affect the results.

powder where the men were burned, in both directions in the cross workings, in the accident above cited. The influence of coal dust in increasing the distance to which the flame from a blown-out shot will extend in mine-workings is therefore conclusively demonstrated by a comparison of the effects of those accidents with the foregoing experimental data. On the other hand, the important circumstance noticed by Mr. Hall that no signs of burning on the props in the mine were visible at greater distances than a yard or two beyond the spots where the men were waiting, although there were open workings in both directions for some considerable distance, and although the flame was sufficiently extensive at those spots to injure the men severely, proved conclusively that coal dust had not the power, in these two instances, to carry on the flame to a great distance from the source of fire. Had there been any gas in the air of the mine the flame would doubtless have extended much farther, and perhaps throughout the adjacent workings. The amount of dust raised by the blown-out shots may, however, have been less considerable than in other similar occurrences, and the dust itself may not have been so highly inflammable, or otherwise of so suitable a character for carrying on flame, as that existing in other mines where undoubtedly dust has played an important part in enhancing the magnitude of explosions. At any rate these results demonstrate the necessity for the exercise of caution in drawing conclusions of too sweeping a nature with regard to the causes and the extent of such coal-mine explosions as cannot be quite clearly ascribed to fire-damp. A few experiments have been made, in the largest gallery (Caponier) at Chatham, to test the power of coal dust to carry on the flame from a blown-out shot. A large quantity of very fine and inflammable coal dust, from Seaham collieries, was suspended in the air by employment of sufficient mechanical contrivances, and clouds of the same dust were also blown into the gallery in the direction of the shot, and immediately in front of it, just when it was fired. One of the frame-screens was placed across the gallery where the pier jutted out (at a distance of 34 feet from the shot), and pieces of guncotton were attached to nails driven in the wall along the short narrow part of the straight gallery and to some distance round the curve. In every one of the experiments tried (three) with $1\frac{1}{2}$ lb. of powder, fired when dust was thickly suspended and carried along in the air, the flame burned a number of pieces of guncotton on the screen; in two experiments guncotton was burned at a further distance of 1 ft. 6 in., but not beyond; in the third, some flame travelled to the end of the straight gallery, and to a distance of 4 ft. 8 in. beyond the curve, but guncotton was not inflamed beyond that point. In this case, therefore, flame reached rather more than, and in the others not quite, double the distance with dust thickly suspended in the air, to what it did in the absence of dust. Experiments will be continued in the long narrow galleries which have been spoken of.

It must now be accepted as beyond question that very few, if

any explosions have occurred of which the destructive effects, so far as burning and production of the fatal after-damp are concerned, have not been more or less considerably increased through the agency of the coal dust raised by the explosion, and that the latter has been in very many cases instrumental in causing the burning effects of the explosion to spread over great areas, and to reach to workings which, in the absence of dust, would have escaped the visitation. Even of late years, long since the observations of Faraday and Lyell have been confirmed and extended, mining engineers and others immediately connected with the working of coal-mines have been very prone to ascribe explosions, which did not admit of satisfactory explanation by an accidental failure of ventilation or other evident causes, to the sudden disengagement or outbursts of fire-damp, such as are, in fiery coal seams, of no uncommon occurrence, and sometimes very serious in their magnitude and long continuance, and to charge such sudden escapes of gas into some part of the mine-workings with the whole extent of the disaster, rather than to credit coal dust with any important share in the origination or even in the extension of the explosion. In many instances the occurrence of such outbursts, following upon falls of roofs or the firing of shots, or the rapid disengagement of fire-damp from coal or goaves, consequent upon sudden changes in atmospheric pressure, have been clearly proved to have preceded disastrous explosions; in others, however, the conclusion that an explosion has been connected with the occurrence of a sudden disengagement of gas in considerable volume, has been based upon assumptions or conjectures, more or less admissible, or upon evidence of doubtful nature collected after the explosion (as in the case of the recent explosion at Seaham Collieries). Under any such circumstance, however, it is, to say the least, extremely difficult to realise how sufficient gas to produce an explosive atmosphere can be conveyed, even by the most powerful ventilating currents which can circulate in mines, from the seat of such a sudden outburst to far distant portions of the mine to which the actual explosion is proved to have extended, within the period which is known, or believed, to have intervened between the first disengagement of the gas and the firing of the explosive atmosphere produced thereby *in the vicinity* of the outburst, by the firing of a shot, by a defective lamp, or by other means of ignition. On the other hand, the character of the effects which in many instances have been produced by the explosion; the evidences of severe burning such as could not be produced by the rapid explosion of a gas mixture only, and the deposition of partially burned or coked dust in very distant and distinct parts of the mine-workings, leave no room for doubt that coal dust has played a more or less important part in almost all the explosions which have been of late submitted to investigation. Further, it must be conceded that in some instances, coal dust would indeed appear to have been the chief instrument of destruction.

To sum up; it has not been difficult, as will have been seen from

the foregoing, to demonstrate experimentally that the existence of a very small proportion of fire-damp in the air of a mine may determine the propagation of flame by coal dust, ignited by the explosion of some local accumulation of a gas mixture, or by the inflammation of gas suddenly disengaged, or even by the flash from a blown-out shot. It has also been clearly established that in so-called fiery mines the air is never likely to be actually free from fire-damp, and that as much as 2 per cent. may exist in the return air of a very efficiently ventilated mine of that class. It must therefore be regarded as a thoroughly well-grounded conclusion that, in many disastrous explosions, coal dust is the chief agent of destruction, and it is indisputable that but few explosions occur of which the effects have not been more or less considerably extended and aggravated by the coal-dust which is raised by the fire-damp explosion. It may also be admitted as not improbable that in some instances, the influence of dust may, apart from its combustibility (as described), determine the ignition of a mixture of air and dust with a small proportion of fire-damp, by the flame which a blown-out shot, or the accidental ignition of some local accumulation of explosive gas mixture has produced. Lastly, it is conceivable, as contended by Friere Marreco, Galloway, and some continental observers, that a mixture of an inflammable coal dust and air, may even, in the complete absence of fire-damp, both originate and carry on to some distance, explosions which, though much inferior in violence to those developed through the agency of gas mixtures, will be at least equal to them in regard to the disastrous effects on the lives of those exposed to them. That mixtures of coal dust and air alone may have the power to carry on the explosion originally caused and disseminated by a gas, air, and dust mixture, into regions where no gas whatever exists, will now be generally admitted. The great disturbance of the air which must proceed in immediate advance of the rush of flame produced by the ignition of a mixture of gas and air charged with coal dust, will, in many mine-workings, raise a dense cloud immediately in front of the flame, and the latter will thus be fed as it advances. Mr. Galloway concludes, as the final result of his experiments with coal dust, that the presence of fire-damp is altogether unnecessary to *bring about* a coal-mine explosion, but, admitting that the result of certain experiments may seem to favour this conclusion, its realisation necessitates the fulfilment of conditions which cannot but be very exceptional, and its acceptance is certainly unnecessary to add to the formidable character of coal dust as a source of danger and an agent of destruction in mines.

Whether an explosion originates with, or is chiefly caused by, the production of a mixture of fire-damp with air in such proportions as to be more or less rapidly and violently explosive; whether the originating cause be the reciprocal influence of a small proportion of fire-damp and of coal dust (or dust of other descriptions of minerals occurring in coal mines) co-existing in the air of a mine; whether, possibly, it simply originates with a mixture of very inflammable coal-

dust and air in the complete absence of fire-damp, or whether, lastly, only the very limited concession be made that coal dust will add to the extent, and increase the burning effect, of a fire-damp explosion; in any case, the existence of dust in abundance, and in a dry state, in coal-mine workings, must be recognised as a source of danger not greatly inferior to that caused by local accumulations, or the accidental liberation, of fire-damp. The possibility of dealing with this source of danger should therefore be as much an object of earnest work as has been the improvement of ventilating arrangements for mines.

It being generally impracticable effectually to deal, by actual removal, with the continual accumulation of dust in mine-workings, the only available method of diminishing the dangers arising from its constant production appears to be that of maintaining the floor in the roads, &c., in a damp condition by efficient watering arrangements, almost continually applied. The high temperature of the mine, in many instances, must often render this a difficult and costly process, on account of the rapidity with which the water will evaporate; hence attempts have been made to apply hygroscopic substances (such as calcium chloride, sea-salt, or rock-salt) in conjunction with water, or to use brine, with a view to retard its evaporation, and some successful results appear to have recently attended their application in several districts. In some instances, with improved appliances for the uniform and periodical distribution of sufficient water, the maintenance of mine-roads in a sufficiently damp condition to prevent dust from being raised in any considerable quantity appears to have been accomplished with fair success; there are, however, localities where it is almost impracticable to maintain the floor of the roads in a damp condition, in consequence of the great increase thereby of the tendency to their being gradually raised by the pressure to which they are subject.

Apart from the effects of dust in augmenting the disastrous results of such fire-damp explosions as may arise from the existence of a defective, or an open safety lamp in the vicinity of an accumulation of gas, or of a locality where a sudden outburst of gas occurs, the *blasting* of coal or of rock, in those parts of a mine where fire-damp may exist, if even only in very small quantities, constitutes the chief source of accidents in which coal dust may have played an important share. There is no doubt, therefore, that the elaboration of really safe methods of getting coal in places where blasting by powder is now resorted to, and of removing the harder rock in the working of drifts where fire-damp may exist, will most importantly contribute towards the diminution of danger arising from the accumulation of dust in mines. The substitution of efficient coal-cutting machines for blasting may to some extent supplant the use of powder, and the employment of compressed air as an agent for bringing down coal or rock has been made the subject of ingenious contrivances, which appear, however, as yet, to labour under some disadvantages in regard

to cost, facility of use and general efficiency. Attempts have been made to render the employment of powder in the presence of fire-damp safe, by using it in conjunction with water. In the first instance it was proposed by Dr. Macnab to bring the latter into direct operation as the cleaving or blasting agent by inserting a cylinder containing water into the blast hole, and connecting it with a very strong external vessel, in which the powder charge was fired, much as the powder charge is fired in the powder chamber of a gun, the generating gas being brought to bear upon the confined column of water, and causing the latter to exert a rending force upon the coal by which it was surrounded. As the results furnished by this method of operation were not promising. the comparatively very simple expedient was resorted to by Dr. Macnab of employing water simply as tamping in a charge hole, a cylinder containing the liquid and of suitable length to fill the hole, being inserted over the charge of powder. In the event of a charge blowing out, the dispersion of the water in a very finely divided condition was relied upon to effect the extinction of the volume of flame which, under these conditions, would be projected into the air of the mine. Some carefully conducted experiments, with blast holes charged by this method and surrounded by an explosive gas-mixture, showed that occasionally no ignition of the gas resulted from the blowing out of the shot, but that in most instances, the conditions of the experiments being the same, the gas-mixture in front of the blast hole was exploded, when the shot blew out. It is possible that a careful regulation of the charge and length of tamping may render this mode of operation a comparatively safe one, though it may be doubtful whether absolute reliance could be placed upon the invariable extinction of flame in the case of blown-out charges. When the attention of the Royal Commission was directed to the subject of the dangers attending the employment of explosives in coal mines, it occurred to Mr. Abel to attempt the application to the getting of coal of the principle which he developed some years ago, in the course of his researches on explosive agents, namely, the sudden transmission in all directions of the force exerted instantaneously by a *detonation*, by surrounding the detonating charge with water. It was found in a large number of experiments that when comparatively small charges of guncotton or dynamite (the latter being preferable) were enclosed in cylinders of light metal or paper filled with water, and occupying the entire available space (or nearly so) in a blast hole, the detonation of the charge in holes of excessive strength, when employed in proper proportion to the amount of water by which it was surrounded, was always accomplished without ignition of the explosive gas-mixture with which the opening of the blast hole was surrounded. The interesting fact was moreover established, by operations carried out in hard coal in Lancashire, that the action of the detonating charge is modified to great advantage, by enclosing the envelope in a long column of water. Instead of exerting a powerfully crushing or disintegrating action, confined within comparatively narrow limits,

whereby a charge of guncotton or dynamite is rendered of little value as a means of getting coal when used in the ordinary way, the distribution of the explosive force in all directions by the column of water causes it to exert a cleaving or splitting action even superior to that exercised by ordinary blasting powder. The farther development of this method of applying detonating agents to blasting purposes in coal-mine workings appears therefore well worthy of attention.

Another method of getting coal, which, though not new in itself, has been applied in a novel manner and with most promising results by Messrs. Smith and Moore, has the great advantage of dispensing entirely with the use of explosive agents, and of any but the most simple mechanical appliances.

It consists in applying the force which quicklime will develop if confined, and made to combine under that condition with water, whereby it undergoes very considerable expansion, a large amount of heat being at the same time developed. Messrs. Smith and Moore convert the freshly burned and crushed quicklime into very compact cylindrical masses, or cartridges, having a small groove on one side, so that when the requisite number of cylinders are inserted symmetrically into the mechanically drilled hole in the coal, which they fit accurately, a narrow pipe, with perforations along its entire length, enclosed in a tight-fitting stocking of open webbing, and provided with a stopcock, may be inserted into the side of the charge, which is afterwards tamped in the usual manner. The proportion of water necessary to slake the lime, *plus* an excess of about one-sixth, is then forced into the hole through the pipe by means of a simple hand syringe, and the stopcock of the pipe being closed, the operation is complete. In a brief space of time sounds indicative of the cracking of the mass of coal which contains the cartridge show that the expansion of the lime by its union with the water, and the very considerable development of steam within the cartridges, are performing their work, and after an interval of time varying with the strength of the part of the seam operated upon, the coal is detached in large blocks. The holes can be charged so rapidly that a considerable number may be put into operation in quick succession by one or two men.* As the action of the charge occupies some little time (fifteen or twenty minutes), they really come into operation together, and in this way large faces of hard coal, in long-wall workings, are brought down with ease and certainty. Whether these compressed lime cartridges can be applied with any success in stone still remains to be determined, but in point of cost, simplicity,

* In one of several operations of this kind recently witnessed by the Lecturer at Shipley Collieries, Derby, in the "deep hard seam," which is nearly 3 ft. thick, ten shots were fired together, bringing down a block of coal 39 ft. long by 3 ft. thick and 2 ft. 10 in. high, weighing about ten tons. The average time occupied in boring a hole (by mechanical drill), charging and tamping it, and watering the charge, was twenty minutes. The usual operation of bringing down this very hard coal, by wedging, is exceedingly slow and laborious.

and above all, safety, this method of detaching coal appears to rank before any other yet tried. Besides entirely avoiding the use and production of flame or fire in the blasting of the coal, the operation is conducted gradually and almost noiselessly, and the raising of dust by the more or less violent concussions which attend the employment of explosives in any form or manner, is avoided.

It is insisted upon by a great majority of those most competent to judge, that the employment of explosives cannot be dispensed with in the profitable working of coal mines. That the use of gunpowder in the ordinary way, even with strict attention to all practicable precautions, is a most prolific source of accident, has long been recognised. The development of safe methods of applying explosive agents, or of simple and effective substitutes for them, is therefore of such paramount importance in securing protection to the miner against the dangers of fire-damp and of coal dust, that those who are entrusted with the management of coal mines should spare no exertions to test rigorously but fairly the merits of any proposals which afford promise of success in this direction.

[F. A. A.]

ANNUAL MEETING,

Monday, May 1, 1882.

THOMAS BOYCOTT, M.D. F.L.S. Manager, in the Chair.

The Annual Report of the Committee of Visitors for the year 1881, testifying to the continued prosperity and efficient management of the Institution, was read and adopted. The Real and Funded Property now amounts to above 85,400*l.*, entirely derived from the Contributions and Donations of the Members.

Fifty-two new Members paid their Admission Fees in 1881.

Sixty-two Lectures and Nineteen Friday Evening Discourses were delivered in 1881.

The Books and Pamphlets presented in 1881 amounted to about 270 volumes, making, with 623 volumes (including Periodicals bound) purchased by the Managers, a total of 893 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Honorary Secretaries, Warren De La Rue, Esq. and William Bowman, Esq. to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

PRESIDENT—The Duke of Northumberland, D.C.L. LL.D.
TREASURER—George Busk, Esq. F.R.S.
SECRETARY—William Bowman, Esq. LL.D. F.R.S.

MANAGERS.

Right Hon. Robert Bourke, M.P.
Thomas Boycott, M.D. F.L.S.
Joseph Brown, Esq. Q.C.
Warren De La Rue, Esq. M.A. D.C.L. F.R.S.
Colonel James Augustus Grant, C.B. C.S.I. F.R.S.
Hon. Sir William R. Grove, M.A. D.C.L. LL.D. F.R.S.
Right Hon. The Lord Claud Hamilton, J.P.
Cæsar Henry Hawkins, Esq. F.R.S. F.R.C.S.
Sir John Hawkshaw, F.R.S. F.G.S.
William Huggins, Esq. D.C.L. F.R.S.
John Fletcher Moulton, Esq. M.A. F.R.S.
Sir Frederick Pollock, Bart. M.A.
Henry Pollock, Esq.
John Rae, M.D. LL.D. F.R.S.
William Spottiswoode, Esq. M.A. D.C.L. Pres. R.S.

VISITORS.

John Birkett, Esq. F.L.S. F.R.C.S.
Charles James Busk, Esq.
George Frederick Chambers, Esq. F.R.A.S.
Frank Crisp, Esq. LL.B. B.A. F.L.S.
Henry Herbert Stephen Croft, Esq. M.A.
Alexander John Ellis, Esq. B.A. F.S.A. F.R.S.
Charles Lyall, Esq.
Robert Mann, M.D. F.R.C.S.
Henry Maudsley, M.D.
William Henry Michael, Esq. Q.C.
Hugo W. Müller, Esq. Ph.D. F.R.S.
Lachlan Mackintosh Rate, Esq. M.A.
The Hon. Rollo Russell, F.M.S.
John Bell Sedgwick, Esq. F.R.G.S.
George Andrew Spottiswoode, Esq.

WEEKLY EVENING MEETING,

Friday, May 5, 1882.

WARREN DE LA RUE, Esq. M.A. D.C.L. F.R.S. Vice-President, in the Chair.

PROFESSOR R. GRANT, M.A. LL.D. F.R.S.

The Proper Motions of the Stars.

THE spectacle presented by the stellar heavens as viewed by ordinary observers is characterised by two remarkable features, the absence of uniformity in the brightness, and the absence of uniformity in the distribution of the stars. Certain of the stars soon came to be recognisable by their superior lustre, and certain groups of stars became familiarly known as so many landmarks in the stellar firmament. The way was thus prepared for an important discovery. It was ascertained respecting a limited number of the stars that their places in the heavens relatively to the general multitude of the stars were continually changing. They consequently received the appellation of planets, or wandering stars, while, on the other hand, the stars in general, in consequence of their always maintaining the same relative position, were denominated *fixed* stars. Ptolemy, in his great work upon the astronomy of the ancients, places the earth in the centre of the universe, and assumes the sun, moon, and planets to be revolving in orbits around it, while beyond all was the sphere of the fixed stars, which revolved with a uniform motion around the earth, effecting a complete revolution once in every twenty-four hours. No opinion is expressed respecting the nature of the stars, nor is any allusion made to the possibility of the stars being endued with a proper motion.

When Copernicus propounded the true system of the universe, he made the earth a planet revolving like the other planets round the sun, and he explained the phenomenon of the diurnal revolution of the starry sphere by the revolution of the earth upon a fixed axis in the opposite direction. No opinion was expressed by him respecting the physical nature of the celestial bodies, or their having any probable community with the earth in this respect. Indeed, it could hardly be said that any new light was thrown upon the physics of astronomy by the theory of Copernicus. As a mathematical exposition of the movements of the celestial bodies it was eminently successful. Indeed, it wanted only the discoveries of Kepler respecting the elliptical movements of the planets to make it perfect in this respect. But it must be acknowledged that in the system

propounded by Copernicus the earth was regarded as the body of paramount importance in the universe.

It was the invention of the telescope and its application to the purposes of astronomical observation which first revealed to the human mind the marvellous extent of the physical universe, and suggested the idea that the earth might be a mere atom in comparison with the vastness of the material system beyond. When it was discovered that the planets are round dark bodies like the earth, shining only by the reflected light of the sun, and that they presented apparent diameters of sensible magnitude when viewed through the telescope, no doubt was henceforward entertained that the planets are bodies comparable with the earth in magnitude, and that the earth is merely one of a family of similar bodies, which revolve in orbits of different magnitudes around the sun. It is worthy of remark that Galileo, to whom is due the telescopic discoveries which first disclosed the vast extent of the material universe, has nowhere expressed any opinion respecting the nature of the stars. His mind was probably too much occupied with the more immediate consequences of his discoveries to indulge in speculations leading to more remote conclusions; and a similar remark is generally applicable to his successors in the field of telescopic exploration, who flourished during the seventeenth century. It was reserved for Huyghens to propound the doctrine that the stars are suns. This he did in a work on Cosmical Astronomy, which was published in 1699, shortly after his death. Henceforward the stars have been regarded by astronomers as self-luminous bodies, comparable in magnitude and splendour with the sun.

While more correct ideas were being formed respecting the nature of the stars, the method for ascertaining the exact position of an object in the celestial sphere underwent at the same time a complete revolution. The telescope in its original form was not suited for aiding the observer in fixing the precise position of a star in the heavens, but the subsequent form of the telescope, consisting of a combination of two convex lenses, suggested the admirable invention of telescopic sights, which may be said to constitute the foundation of all exact astronomy. The places of the stars were now determined with a vastly greater degree of precision, and the way was thus prepared for the consideration of the important question whether the epithet *fixed* is strictly applicable to those bodies, or whether they might be rather endued with a movement, so extremely slow as to have hitherto eluded detection.

To Halley is due the discovery of the important fact that some of the stars have a proper motion. In 1717 he communicated a paper to the Royal Society, in which he showed that a comparison of the places of Sirius, Arcturus, and Aldebaran, as determined by Hipparchus about the year 130 B.C., with corresponding observations of the same stars made by himself, clearly indicated that during the intermediate interval the stars had sensibly moved southwards with

respect to the ecliptic, and he obtained a further confirmation of this result by examining the account of an occultation of Aldebaran by the moon, observed at Athens in the year 509 A.D.

A few years after Halley announced this important fact, Bradley made his famous discovery of the aberration of light, and its effect upon the apparent place of a star; and subsequently the same astronomer discovered the apparent sidereal movement depending on the nutation of the earth's axis. The astronomer could now ascertain the true place of a star in the heavens with a precision to which the results of previous efforts could offer no comparison, and it seemed probable that ere long the great problem of the proper motions of the stars might be attacked with some hope of success.

To ascertain the proper motion of a star it is necessary to have two well determined places of the star separated from each other by a sufficiently great interval of time. Down to the middle of the last century no such materials may be said to have existed, if we except a few isolated cases such as those referred to by Halley, for the probable errors in the observed places of a star far exceed in magnitude the minute quantity which was the object of inquiry. To Bradley is due a great work of observational astronomy which has constituted the basis of the more extensive investigations of the present day relating to the proper motions of the stars. This consisted in a series of star observations executed by that astronomer at the Royal Observatory, Greenwich, from 1750 to 1762, but which it was reserved for Bessel, the great German astronomer, to reduce, and finally to publish in the year 1818. A comparison of those star places with the corresponding results obtained at the Greenwich Observatory in the present century by Sir George Airy, the late Astronomer Royal, has conducted astronomers to important conclusions respecting the proper motions of the stars. Materials tending to elucidate the same great question have also been derived from the star observations of several other astronomers of the present century.

[The lecturer here exhibited a diagram containing the following illustrations of the proper motions of the stars:—

Star.	Magnitude.	Proper Motion in a Thousand Years.
		″
Sirius	1	1360
Procyon	1	1210
Arcturus	1	2230
α Centauri	1	3710
Capella	1	250
Rigel	1	20
Antares	1	30
Groombridge, 1830	7	7106
61 Cygni	6	3200
O² Eridani	4	4100
Lalande, 27,744	6	1681
Lalande, 30,044	7	1607
Lalande, 30,694	6	1789
Weisse's Bessel XVII., 322	7	1476]

The last four proper motions have been recently detected at the Glasgow Observatory, where a system of star observing has been prosecuted since the year 1860.

It must strike every one who inspects the foregoing list that the proper motion of a star has no relation whatever to the apparent magnitude of the star. Thus Rigel, one of the most brilliant stars in the heavens, has a proper motion of only 20″ in a thousand years. On the other hand, the star 1830 Groombridge, which has a proper motion of 7106″ in a thousand years, is a star of only the seventh magnitude. The same remark obviously applies to the other stars in the list. And yet one would have thought that the brighter stars, being presumably nearer to us than the fainter stars, would for that reason have a larger proper motion. With respect to *a* Centauri and 61 Cygni, which we know, from the researches of astronomers on their parallax, to be the two nearest stars, it turns out conformably to what one might expect, that they have also large proper motions; but what are we to think of 1830 Groombridge, which, although a star of only the seventh magnitude, and one which hardly indicates any sensible parallax, exhibits notwithstanding the largest proper motion of any star in the heavens? These anomalies are doubtless attributable to differences in the absolute magnitude and intrinsic splendour of the stars, and furthermore to the fact that the proper motions as revealed by the telescope are only the motions which are resolved at right angles to the line of sight.

Heretofore the proper motion of a star has been found to take place constantly in the same direction, and as the angular amount of proper motion is in all cases exceedingly small, the same result will probably continue to manifest itself for ages to come. The mean apparent diameter of the sun amounts to 1944″, consequently Arcturus would require nearly a thousand years to describe, in virtue of his proper motion, an arc of a great circle of the celestial sphere, equal to the mean apparent diameter of the sun.

The lecturer next adverted to the interesting spectroscopic researches of Huggins, and Christie the present Astronomer Royal, on the proper motions of the stars in the direction of the line of sight, and he concluded with some remarks on the great problem of the motion of the solar system in space.

[R. G.]

GENERAL MONTHLY MEETING,

Monday, May 8, 1882.

WILLIAM SPOTTISWOODE, Esq. M.A. D.C.L. Pres. R.S. Manager, in the Chair.

Robert Cordy Baxter, Esq.
George Christian, Esq.
Charles Combe, Esq.
Carl Haag, Esq.
John J. Edwin Mayall, Esq. F.C.S.
Alfred Meadows, M.D.
Mrs. Charles W. Mitchell,
Colonel Francis Richard Waldo Sibthorp,
Alexander Siemens, Esq.
Mrs. Alexander Siemens,
Arthur John Wright, Esq.

were elected Members of the Royal Institution.

JOHN TYNDALL, Esq. D.C.L. LL.D. F.R.S. was re-elected Professor of Natural Philosophy.

Two Candidates for Membership were proposed for election.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Accademia dei Lincei, Reale, Roma—Atti, Serie Terza: Vol. VI. Fasc. 9, 10. 4to. 1882.
Actuaries, Institute of—Journal, No. 125. 8vo. 1882.
Asiatic Society of Bengal—Proceedings, No. 10, 1881. No. 1. 8vo. 1882.
Asiatic Society, Royal—Journal, Vol. XIV. Part 2. 8vo. 1882.
Astronomical Society, Royal—Monthly Notices, Vol. XLII. No. 5. 8vo. 1882.
Banker's Institute—Journal, Vol. III. Part 4. 8vo. 1882.
Batavia Observatory—Rainfall in the East Indian Archipelago, 1881. By P. A. Bergsma, the Director. 8vo. Batavia, 1882.
Bavarian Academy of Sciences, Royal—Sitzungsberichte: 1882, Heft 2. 8vo.
British Architects, Royal Institute of—Proceedings, 1881–2, Nos. 13, 14. 4to. 1881–2.
Canada Geological and Natural History Survey—Report of Progress for 1879–1880, with Maps. 8vo. 1881.
Chemical Society—Journal for April, 1882. 8vo.
Civil Engineers' Institution—Minutes of Proceedings, Vol. LXVII. 8vo. 1881–2.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Series II. Vol. II. Part 2. 8vo. 1882.
Dax: Société de Borda—Bulletins, 2e Série, Septième Année: Trimestre 1. 8vo. Dax, 1882.
Editors—American Journal of Science for April, 1882. 8vo.
Analyst for April, 1882. 8vo.
Athenæum for April, 1882. 4to.
Chemical News for April, 1882. 4to.
Engineer for April, 1882. fol.

Horological Journal for April, 1882. 8vo.
Iron for April, 1882. 4to.
Nature for April, 1882. 4to.
Revue Scientifique and Revue Politique et Littéraire for April, 1882. 4to.
Telegraphic Journal for April, 1882. fol.
Franklin Institute—Journal, No. 676. 8vo. 1882.
Geographical Society, Royal—Proceedings, New Series, Vol. IV. No. 5. 8vo. 1882.
Geological Society—Abstracts of Proceedings, 1881-2, Nos. 419, 420. 8vo.
Glasgow Philosophical Society—Reports on Exhibition of Apparatus for Utilization of Gas, Electricity, &c. Sept. Oct. 1880. 8vo. 1882.
Hudleston, W. H. Esq. M.A. F.G.S. M.R.I. (*the Author*)—On Deep Sea Investigation. 8vo. 1882.
Iron and Steel Institute—Journal for 1879, 1880, and 1881. 8vo.
Linnean Society—Journal, Nos. 92, 117–119. 8vo. 1882.
Lisbon, Sociedade de Geographia—Boletim, 2ª Serie, Nos. 9, 10. 8vo. 1881.
MacCormac, Sir William (*on behalf of the Council*)—Transactions of the International Medical Congress, Aug. 1881. 4 vols. 8vo. 1881.
Mechanical Engineers' Institution—Proceedings, No. 1. 8vo. 1882.
Medical and Chirurgical Society—Proceedings, Part 54. 8vo. 1882.
Meteorological Office—Communications from the International Polar Commission. Part 2. 4to. St. Petersburg, 1882.
Meteorological Society—Quarterly Journal, No. 41. 8vo. 1882.
National Association for Social Science—Transactions, Dublin, 1881. 8vo. 1882.
Proceedings, Vol. XV. No. 5. 8vo. 1882.
Numismatic Society—Numismatic Chronicle and Journal. 3rd Series. Nos. 1–4. 8vo. 1881.
Pharmaceutical Society of Great Britain—Journal, April, 1882. 8vo.
Photographic Society—Journal, New Series, Vol. VI. No. 7. 8vo. 1882.
Royal Society of Edinburgh—Transactions, Vol. XXX. Part 1. 4to. 1880–1.
Proceedings, Vol. XI. 8vo. 1880–1.
Scottish Society of Arts, Royal—Transactions, Vol. X. Part 4. 8vo. 1882.
Society of Arts—Journal, April, 1882. 8vo.
Statham, H. H. Esq. (*the Author*)—Notes on Ornament. (Lectures delivered at the Royal Institution.) (Portfolio, April, 1882.)
Statistical Society—Journal, Vol. XLV. Part I. 8vo. 1882.
St. Pétersbourg, Académie des Sciences—Mémoires, Tome XXIX. Nos. 3, 4. Tome XXX. Nos. 1, 2. 4to. 1881–2.
Symons, G. J.—Monthly Meteorological Magazine, April, 1882. 8vo.
Telegraph Engineers, Society of—Vol. XI. No. 41. 8vo. 1882.
University of London—Calendar for 1882. 12mo. 1882.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1882: No. 3. 4to.
Victoria Institute—Journal, No. 61. 8vo. 1882.
Zoological Society—Transactions, Vol. XI. Part 6. 4to. 1882.
Index to Transactions, Vols. I.–X. 4to. 1882.
Proceedings, 1881, Part 4. 8vo. 1882.

WEEKLY EVENING MEETING,

Friday, May 12, 1882.

Sir Frederick Bramwell, F.R.S. in the Chair.

A. G. Vernon Harcourt, Esq. M.A. F.R.S.

The Relative Value of Different Modes of Lighting.

(Abstract deferred.)

WEEKLY EVENING MEETING,

Friday, May 19, 1882.

William Bowman, Esq. LL.D. F.R.S. Honorary Secretary and Vice-President, in the Chair.

Sir Frederick Bramwell, F.R.S. *M.R.I.*

The Making and Working of a Channel Tunnel.

Such is the title of my lecture this evening, and you will gather from it that I come before you with practically an abstract proposition; thus I do not ask you to consider with me whether the tunnel should be made at all, whether, if made, it would be a pecuniary success in the way of earning a large revenue, or whether, if made, it would detract in any appreciable degree from the safety of our insular position. These are contentious matters, and these are therefore purposely omitted from my lecture of to-night.

We will leave for the Society of Arts the consideration of the question, whether those men who are willing to embark their money in such an undertaking, are wise or not, and to it, and to the Statistical Society we will leave the question, whether persons who base their estimates of revenue upon existing traffic, and do not allow for the development arising from an improved mode of communication, are as unwise as those who, in the early days of railways, opposed them on the ground that they never could pay, because, as those persons showed, the stage-coach and the canal-barge transported only *so many* passengers and *so many* tons, supplemented by the tonnage of the road waggons, and they argued that the fares and freights to be earned for the transportation of the whole of these passengers and tons, so far from yielding a dividend, must be utterly inadequate to cover working expenses.

We will leave these Societies jointly to consider whether, if it be thought desirable to make a bridge with spans of nearly a third of a mile in length, and of a height of 150 feet above the water, and 450 feet from the extreme top of the piers to the bottom of the foundations, over the Firth of Forth, to connect up a population of only about a million north of that Firth with the country south of it, and to expend two millions sterling on such a bridge, it is desirable to improve the means of communication, and render them free from all perils and questions of the sea, between the whole of Great Britain, with its thirty millions of population and its wondrous manufacturing and productive power, and the continent of Europe, and through that continent with our possessions in India and Australia. Finally, we will leave it to the United Service Institution, guided in their delibera-

tions by the Report which will, I presume, soon be issued by the Commission appointed for the purpose, to consider, whether, by the making of two covered ravines 20 miles in length, and having each of them a width of only 14 feet, the safety of our insular position, diminished as that safety is in these days of rapid steam propulsion and transportation, would be affected in any appreciable degree.

I am afraid, that notwithstanding my guarded utterances, you may have gathered what my views are on those points which I am not to-night to enter upon. If, however, any here have not so gathered them, I can only say to those who may be interested in knowing what my opinion is, that in the right places and at the right times, I am fully prepared to clearly state my views, and to give reasons for the faith that is in me concerning them.

In the interest of a Channel tunnel, what a happy circumstance it was, that the unknown platelayer of the Stockton and Darlington Railway, who took there the gauge he had used in the colliery lines, had not, at about the same time, imitators in France, and in the different countries of the Continent. No doubt, if in these countries the development of railways had been made simultaneously, by their engineers, with the development of railways in England, we should have had some highly scientific fraction of an imaginary section of the earth's surface—a fraction containing probably six places of decimals and some repeaters—acknowledging the want of absolute accuracy in the decimal quantity—adopted as the gauge of the lines of railway in France, another one in Germany, and a third in Italy. But luckily, when railways spread on the Continent, they spread from England, and they spread by means of English engineers, and before our Continental friends knew where they were they found themselves employing the 4 feet $8\frac{1}{2}$ inch gauge, with which the railways in this country are, as a rule, laid. So it is, that, excluding the Peninsula and Russia, a railway carriage may start from Calais and go over the whole continent of Europe; and so it will be, when a Channel tunnel is made, that a railway carriage may start from Thurso or Wick, and passing through London, may join on to the train from Charing Cross, traverse the tunnel, and continue without any break of gauge, or trans-shipment of passengers, through France, Italy, and Germany.

Having premised this much, I will at once pass to the first of the two divisions of my lecture—*The Making of a Channel Tunnel.*

Those who have studied the subject of tunnel-making will be aware that, as a rule, tunnels either have to be carried through soils of so loose a character, that they cannot stand for a moment without support, or they have to be executed in rock, more or less hard, often much harder than can be dealt with by hand labour. In this latter case, the driving forward is effected by blasting, an operation involving either great manual labour for the preparation of the holes into which the blasting charges are to be put, or, in later days, involving the employment of machinery (worked by compressed air) to make

these holes. These are, popularly speaking, the two great divisions of tunnel-making.

There is, however, one well-known exceptional case, where a tunnel was driven through an all but fluid material in the bed of the river Thames. I mean Brunel's celebrated tunnel, now used as a portion of the East London Railway.

In ordinary railway tunnelling, such as we meet with in England, it is commonly possible to sink shafts at frequent intervals, and in this way to multiply the number of faces, or ends, at which men can work; but in the case of the great tunnels made of late years under high mountains, such as the Mont Cenis tunnel, 7½ miles long, and the St. Gothard tunnel, 9¼ miles long, it is not possible, having regard to the expense of the deep shafts, and the comparatively inaccessible positions of their mouths, to accelerate the rate of progress or to add to the ventilation by their use, and in such cases it becomes necessary to do the whole of the work from the two ends.

Hitherto I have spoken of railway tunnels. I may, however, be permitted to remind you that the earlier tunnels made in England were those employed for canal purposes; these, however, must not be further referred to, nor will I delay by calling your attention to the great canal tunnel made in France in the year 1681, or to the still earlier tunnel, the Grotto of Posilipo at Naples.

Besides tunnels proper, there are subterranean workings, to which the term tunnel is not applied, but which are undoubtedly as strictly tunnels as are those through which trains pass or along which barges are hauled. You are of course aware that I am alluding to our great underground works for mining purposes. Here, at depths below the surface varying from a few hundred yards to half a mile, and in some few instances extending below the bed of the sea, we have tunnels which even in a single colliery have an aggregate length of many miles, excavated by means of only two shafts, and carried through rock of the most varied character, and too frequently subjected to the dangers arising from the firing of the gases, and from, as we were so well shown a few weeks ago by Mr. Abel, the dangers arising from the explosion of the dust with which these galleries are in many places covered.

As regards stratification, obviously, if it were a matter of choice, neither the unstable earth, nor the clay which, although easily cut, cannot be left for a moment without support by timbering, pending the getting in of the permanent lining, would be selected, neither would be chosen the rock which, although capable of standing alone, demands for its excavation the labour and cost incident to drilling and blasting operations, and demands also the great expenditure of time needed for working under these circumstances.

The perfect material, so far as regards its excavation, would be one, which, while it was capable of being easily cut, was also capable of self-support for a long time, if not in perpetuity; and further, if in addition to these two excellent qualities as regards excavation and

maintenance of form, a material could be found which was practically impermeable to water, I think it must be admitted that this would be the stratification of all others which the engineer would seek, had he the power of selection. But in most cases of tunnelling, still more in the case of a Channel tunnel, the engineer is bound by conditions which, except within very narrow limits, prevent him from selecting his stratification; he is compelled, as a rule, to take such stratification as may occur. If this be true ordinarily of a line of railway through the country, how much more is it true in the case of a Channel tunnel? Looking at the map, we see the Channel, in its extent from the Lizard to the North Foreland, ranging in width from 100 to 60 or 50 miles, except in one place, but at that one place, viz. for the short length between Boulogne and Calais on the French side and Hythe and Dover on the English side, we find the Channel reduced to a width of some 20 miles. Obviously, all other things being equal, it is in this part, on account of its narrowness, that the tunnel should be made, and there are further reasons why this place should be chosen.—This narrower part has been selected from the earliest times for the crossing from side to side, and in that way the roads first, and the railways afterwards, have on the two sides converged towards this part, and thus there exist already the inland communications. As I have said, everything shows the desirability of having the Channel tunnel in this narrow part. Then comes the question, is it at this narrow part that the desirable stratification is to be found? Well, most fortunately we do find, as stated on the highest geological authority, that there is, on the English shore and on the French shore, the very formation of all others which is desired.

Every one knows that on the two sides of the strait there are chalk cliffs, and that this formation extends inland in both countries for some distance. Chalk, we are well aware, is a material readily cut, and when not exposed to the weather, is self-supporting. But, it may be said, how about the water? Is not the chalk the very formation in which water is sought and from which it is obtained in such quantities for the supply of towns? and of myself it may be asked, Are not you one of the engineers who some three or four years ago suggested that so far as regards its potable water, London should be supplied from the chalk? How, therefore, can such a material be one in which to make this tunnel? Further:—it is well known that on the beach in St. Margaret's Bay, and about there, the fresh water wells up from the chalk in volumes. This is all perfectly true, and if there were only one kind of chalk it would be an extremely pertinent criticism; but there is *chalk* and *chalk*. Let me ask you to look at the samples on the table and at the diagrams before you. You will see there the white or upper chalk of which I have just been speaking, seamed with flints and loaded with water, but you will also see depicted the lower chalk (grey chalk), or "craie de Rouen," and the chalk marls.

Equally excellent with the upper chalk for facility of excavation is this lower chalk—indeed, much more excellent, inasmuch as it is free from the beds of flints—equally excellent with the upper chalk for preserving its form, but in another respect infinitely superior to the upper chalk, inasmuch as it is practically watertight—analysis shows that the lower chalk contains much clay mingled with it so as to enter into its composition; it is, in fact, an unburnt natural Portland cement; and you will, by the sort of dirt pie I have formed, see that it is capable of being worked into a perfectly watertight puddle.

Now comes the question, does this material exist right across the Channel at the place where it is intended to make the tunnel, or does it not? The reasons for believing that it exists are that we find it on both sides, and we find it under similar conditions, giving, therefore, good ground for the opinion that there is uniformity between—an opinion strengthened by the fact that the line of outcrop of the gault on the sea-bottom, as determined by sample-taking soundings, is consistent with the indications on the shores and with the uniformity between. No one can say with absolute certainty that there is this uniformity. It can after all be only a matter of opinion, but the probabilities in favour of its existing are so great that reasonable, prudent, business men, guided by the information they receive from scientific geologists, are found to be willing to embark their money in making an experimental heading which shall solve the question; and let it be remembered that if they are wrong the whole loss falls upon themselves, and in addition there comes upon them that which an Englishman dreads more than the loss of his money, i. e. ridicule, the "I told you so's" of his less adventurous friends; but if these prudent business men be right, then I think you will agree they should have a fair profit for their venture. But to whom will the great profit accrue? Not to the makers of the tunnel, but to the community at large. It appears to me, therefore, that, looking upon these makers of the submarine railway on the one side, and the public on the other, it will be seen that the public are engaged in that very safe game wherein while no hurt can happen, great benefit may accrue to them, whereas those who are undertaking the expenditure are so engaged in the game, that while they may suffer the whole of the loss, they can only reap a fraction of the benefit.

Now you will see that this material—this grey chalk—is, like the common chalk, cut with the greatest ease by means of an ordinary pocket knife. No blasting is needed, and no drilling of holes for cartridges. This being so, how shall it be got out—by men working pickaxes and tools of that kind, and making a somewhat irregular-shaped hole? I think it is seen at once this is not the mode, when you bear in mind first that we can have no shafts here, multiplying the places for working, and that thus only the two faces will be available; and next that only a certain number of men can work against a face. Relieving them as often as you will their rate of progress must be slow, the cost considerable, and the need of

ventilation, and the problem of how to ventilate under such circumstances, must be matters of difficulty. Everything points therefore to the employment of machinery; this must however be worked at a distance from the original source of power. I say so because no one would suggest that a steam engine, with its boiler and furnace, should be used in such a position underground, and therefore we must take it that the source of power will be situated on the shore. Now in what manner is the power to be transported? Fifty years ago power was transported to such places by a mode still perfectly practicable and feasible, namely, the making of a partial exhaustion at the source of power by means of air-pumps, exhaust mains connecting these pumps to the engine to be worked at a distance. With this arrangement it will be seen that the pressure of the atmosphere exerted on the pistons of the engines, to the exhaust pipes of which the exhaust mains were connected, produced the requisite power.

This then was the system of conveyance of power by exhaustion, and it had the merit of ventilating the place where it was being worked, not, it is true, by air delivered at the working face, but by air drawn in, if I may so phrase it, from the shaft, and all the way along the heading to the working face to feed the engines which were worked by the exhaustion.

Another mode of conveying power in these days is by the laying on of gas to work a gas motor. It is perfectly feasible, but the result of using this mode would be, that the products of combustion would vitiate the atmosphere nearly as badly as it would be vitiated by the employment of coal-fires underground. This mode therefore is unsatisfactory.

Another mode would be by the conveyance of water under pressure; this has for many years been employed by Sir William Armstrong, and others following him, to transmit power to a distance. Such a system is free from the disadvantage of vitiating the air in any way, but on the other hand it does not contribute to the ventilation. It has, however, when coupled with the improvements made by Mr. Crampton for the special purpose of tunnelling, advantages which I shall describe later on.

The most modern plan of conveying power, is its transmission by electricity; this, like the hydraulic mode, neither detracts from the purity of the air, nor does it add to it.

There remains, however, the converse of the exhaustion system, which is the one commending itself, I think, upon many grounds as being very applicable to the transmission of power to the end of a long tunnel or heading. I mean the mode of working by compressed air.

This is extremely simple in its operation. Driven by a steam engine on the surface there are compressing pumps which draw in the atmospheric air, compress it to the desired degree, and this compressed air is, by means of pipes, carried down the shaft and along

the tunnel to the face where the engine is employed to make the excavation. The compressed air presses on the pistons of these engines in the same manner as that in which steam presses on the piston of a steam engine, but, for the purpose we are now considering, there is this important difference, that whereas the steam engine delivers its exhaust steam into the air, thereby filling it with unbreathable vapour, the compressed air engines deliver the exhaust air into the workings, and in that manner the very machine which is doing the work is, as an accident of this work, effecting the ventilation. It must not be supposed that compressed air engines are any novelty, or that their efficient working is a matter of doubt. They have been employed in various situations, and for underground purposes for many years. They were used, as you probably well know, in the Mont Cenis tunnel, and in the St. Gothard tunnel, for working the drills which bored the holes in which the cartridges for the blasting of the rock were placed. In fact the mode of construction of these engines, and the manner of their using, are as well understood as are the mode of construction, and the manner of use, of the steam engine.

There is known exactly, what would be the loss by friction of the compressed air in passing through a given-sized pipe, a loss varying with the velocity and with the pressure; and upon this point I may say that compressed air has been worked at from as much as 1000 lbs. on the square inch down to as little as 30 or 40 lbs. on the square inch, and I may tell you that a pipe little more than 11 inches diameter would deliver at 10 miles distance air which had originally been at 50 lbs. pressure in quantities sufficient to develop 150 horse-power at the working face, with a loss of pressure of about 25 per cent., while if the air had been brought up to 500 lbs. pressure a pipe a little over 4 inches diameter would enable, at the same percentage of loss of pressure, a similar horse-power to be obtained at the end of 10 miles of such pipe.

Having determined how the machine is to be driven, let us next consider in what manner it shall attack a stratification capable of being easily cut. An obvious way would be to scoop out channels in the face of the work, and to make these of such depth as to enable the block contained between the channels to be broken off and taken away as a whole; but you will see that if this mode were adopted, the work of cutting must be suspended while the block was being removed, and the block must be got out past the machine, and that this would be a matter of difficulty, especially in a small trial heading, and must cause delay.

On the whole, therefore, it becomes much more simple and economical, and much more rapid—and in a case like this rapidity is the true economy—to use such a number of cutters as will cut the whole face of the heading to pieces, making it, as it were, into small fragments, or shavings. A case containing these shavings is before you. The advantage of thus dealing with the material you will perceive

is this—that the cuttings being continuously produced, can be readily carried away by a simple apparatus, a travelling chain of scoops (for which room can be found between the machine and the sides of the excavation), and can be poured, as produced, into waggons waiting to convey them away. In this manner the operation and the progression of the machine may be continuous—with one qualification however—the machine stands upon a bed, along which bed it is capable of forward motion proportioned to the progress made at each revolution; but of course the time comes when after the machine has gone forward to the extent limited by the bed, the end of its travel is, for the time, reached, and then it is necessary to stop the machine, and uphold it, while the bed is moved forward underneath to give it a fresh point of support for a renewed start upon its journey. With this exception however, the advance is, as I have said, continuous.

Now although by the employment of machinery we can, by suitable appliances, make an excavation of almost any figure that may be needed, there is no doubt that the plain circle form is the one which of all others can be the most readily carried out, as for such a shape nothing more is required than a revolving arm, or arms, carrying the cutters upon the face, making thereby an absolutely cylindrical hole of the size of the diameter of the arm. One of the working cutters for the 7-foot hole, which is the size of the trial heading, is now before you.

I may mention, to show the way in which the excavation in this material retains its figure without alteration, that the part first cut out of the trial heading many months ago, is now as truly cylindrical, and is as accurately of the same dimensions, as it was the very day it was made; no change of shape has taken place; in fact, the polish left by the cutter remains upon the circumference.

I am aware that some comments have been made as to the difficulty of getting rid of the quantity of stuff as fast as it is cut, especially, it is said, if it be borne in mind that at the same time materials may have to be taken into the tunnel, for the purpose of lining it, and it is suggested that this, if a difficulty at the outset, and near the ends of the tunnel, will be an increasing difficulty as the tunnel progresses, and the distance to the working face therefore increases.

I will deal at once with this last state of things. Supposing that trains of waggons may work within any stipulated distance the one from the other, say for instance a quarter of a mile, it is obvious that after the tunnel has attained a length of a quarter of a mile, you may then establish a second train, to be running on the rails at the same time as the first one, and when the tunnel has advanced half a mile you may establish a third train, and so on; that is to say, as you attain each successive distance, such as you have determined shall be that which ought to be preserved between the trains, you may then provide another train.

Those who raise this difficulty do not appear to see that it does

not follow because it is ten miles from the shore to the middle of the tunnel, that therefore the number of trains at work at the same time on that ten miles taking materials outwards should be limited to one. And yet no one suggests that upon the Metropolitan and District Railways, for example, there shall only be one train on the line all the way between Aldersgate Street and the Mansion House. As a fact, there are at one and the same time fifteen or twenty trains upon that distance. Similarly there could be any reasonable number of trains upon the line in the tunnel as its length increased.

Let us see, however, what this question of the material to be removed amounts to. Assume that the tunnel for one permanent line is to be 14 feet in diameter internally when finished, and assume that it will be lined with material, about which I shall speak presently, 1 foot 6 inches in thickness, that would make a 17 feet ($5\frac{2}{3}$ yards) diameter excavation, equal to, not quite, 25 superficial yards. Assume the machine to be making a progress of 2 yards in an hour, that would amount to 50 cubic yards, or not more than a sufficient load for one train, and this would be a rate of progress that would advance a mile in the working days of six weeks, so that about fifteen months would finish the ten miles.

It may be said that this is taking too sanguine a view of the speed at which the work would go on; but if this be so no one can suggest that the supposed difficulty of removing the excavated material is being shirked by an under-estimate; but even as thus stated, it needs only one train an hour. Well, it may be said, one train an hour is easy enough on an ordinary railway with locomotives, but how is this one train an hour to be got along underground in a close-ended tunnel, where you won't dare to use an ordinary locomotive? Are you going to do it by horses? The answer is, No! Again the compressed air comes to our aid, and you will find that, without the slightest difficulty, compressed air locomotives, which are already in use in works of this character, and, as I shall have occasion to tell you when I consider the permanent working of the tunnel, have been in lengthened use elsewhere, would be employed for these trains.

I think you must agree with me that there is no practical difficulty about the transport of the material along the tunnel; but, then, the question may be put, how is it to be got to the surface in sufficient quantities?

Two modes are available here. One is that the land tunnel (the incline) should be completed first, and that the material should be got up the incline by a locomotive. But as this waiting for the completion of the land tunnel would involve an unnecessary delay, and as it clearly would be better that the works of the land tunnel and of the part under the sea should go on simultaneously, the material would be got to the surface by winding engines. Let us see what that amounts to. You have 50 cubic yards in an hour for each tunnel, that is, 100 cubic yards in an hour for the two; call the

weight of this 150 tons. The height through which this will have to be lifted is some 60 yards, a distance so slight that there is no reason on the score of expense why there should not be two or three independent shafts, or, better still, one large shaft with two or three separate lifts in it, so that each lift would bring up about 50 tons an hour, or 500 tons in the 10 hours. There are coal-pits where as much as 770 tons in the ten hours are raised, not from a depth of 60 yards, but from a depth of 600 yards, or ten times 60 yards. This suggested difficulty, therefore, it will be seen, has no foundation.

But although, I trust, I have made it abundantly clear that there is no practical difficulty whatever in getting the excavated materials, by the use of ordinary waggons drawn by compressed air engines, to the base of the shaft, nor in raising by winding engines, there is another mode of getting rid of that material, and another system of working, which has been suggested by Mr. Crampton, a mode that well deserves consideration. His proposition is to employ hydraulic power (water under pressure) as the motive agent, to drive either the special cutting machine which he suggests, or any other cutting machine, at the face of the heading or tunnel, and also to drive the pumps necessary to send this water back along the tunnel to a sump at the foot of the shaft, from which it can be pumped up, or to send it by one pumping direct to the surface. He shows that the water, after having worked the engines requisite to drive the cutting machine and the pumps, would, in any given time, be in bulk equal to from five to six times that of the chalk which would be cut in that time. He proposes that the fine shavings of chalk, as they are cut, should be delivered into a mixing machine (of a description similar to that which he has employed for very many years for brick-making purposes), where the waste water from the engines would be united with the shavings of chalk, resulting in the production of a cream containing one part of chalk to six parts of water in volume ; the ordinary cream of a brick-field being one volume of chalk and one volume of water. He then shows, that by this process the whole of the material might be continuously delivered through pipes to the surface, without needing any train accommodation at all, or any winding apparatus. This would leave the rails in the tunnel perfectly free for the trains bringing the material for the lining of the tunnel. I stated at the British Association meeting at York last year that at the pressure ordinarily used by Sir William Armstrong, viz. 700 lbs. on the square inch, 300 horse-power can be conveyed through a 10-inch pipe, with a loss in friction of only 2 per cent. for each mile in length, so that in mid-channel there would be no greater loss than 20 per cent. This plan, having regard to the extreme simplicity of the mode which it affords of getting rid of the excavated material, is, I think, well worthy of consideration, but it must be borne in mind that air pipes would still have to be laid down, and blowing machinery (although not compressing machinery) would have to be erected for the purpose of ventilation,

while when compressed air is used as the motive power, the ventilation is effected by the very spent, or exhaust, air from the machine itself.

Next, as regards the lining of the tunnel. It must be remembered that in excavating through ordinary soil needing lining, heavy timbering has to be put up as the work goes forward, and then the lining has to be executed with the greatest care, following up the work as closely as possible, to enable the timber to be removed. In the case of the Channel tunnel, made of a truly cylindrical form through such a material as the grey chalk, which upholds itself so well that it is doubtful whether there need be any lining at all, no timbering will be required, and there will be no urgency in putting the lining in. Moreover, the excavation being of absolutely uniform dimensions, and of regular shape all round, it will be the simplest thing imaginable to make on the surface concrete blocks of just such size as to be easily lifted by small hydraulic, or compressed air, cranes, and of the form of the 17 feet curvature outside and the 14 feet curvature inside; and without any scaffolding or permanent centering of any kind, with no more than the appropriate elevating apparatus, this lining would be laid in the excavation, beginning at the bottom and working upwards at the two sides, and then over the crown, where it would be supported by travelling centering mounted upon wheels, and moved forward as the work progresses.

Let me ask you to compare in your own minds the condition of things, as regards ease of excavation, economy, and ventilation which would obtain in a tunnel thus constructed with that which obtains in a tunnel such as the St. Gothard, or the Mont Cenis, carried through hard rock. In the tunnel through the chalk, in lieu of men crowded close together at the working face tending upon machines to drill holes to receive cartridges, we have a machine needing the presence of certainly not more than three men at the outside; in lieu of the delays occasioned by the withdrawing of the machinery to admit of the holes being charged with dynamite, and the need of the retreat of the men to a safe place while the dynamite is exploded, and the interference with the ventilation caused by this explosion (coupled with the uncertainty when the explosion has taken place that the shots will have been satisfactory), and the further delay caused by getting out the blasted material to put it into waggons and carry it away; we have continuous action of the machine, we have no blasting and therefore no bad atmosphere produced by it, and no uncertainty as to the effect of the blasts, neither have we any delay in the work while the material is being removed and put into the waggons. Moreover, we are free from all the dangers attendant upon the use of explosives.

Then again, consider our condition as compared with a tunnel through an ordinary non-rocky material: we have not the expense of providing the timber supports, neither do we require a number of men at work to put them up, thus little ventilation will be needed because there will be but few men present to vitiate the air, and as we employ compressed air locomotives to move our trains of materials

there will be no horses; and again, when we come to another source of vitiation, viz. that arising from the lamps and candles ordinarily used in such work, we discard such means of illumination and by the electric light and the incandescent lamp get rid of all that difficulty. The preliminary 7-foot heading is now perfectly lighted, without any vitiation of air whatever, by means of this electric light.

Of course it is obvious, that both during the carrying out the works of the tunnel, and in the permanent tunnel itself, suitable enlargements can be made at intervals to enable sidings to be laid in, depôts to be provided, and places for the plate-layers and others to take refuge in, and I should have said that the approach from the tunnel to the land is to be made by another tunnel having the perfectly workable gradient of 1 in 50.

I am aware it may be objected, that even assuming the geologists are right in their suggestion—that the chalk marl, or grey chalk, does extend from France to England, and that if excavation can be pursued throughout in the "craie de Rouen" the case as to the facility of execution would be established—there is nothing to show that this chalk may not be fissured in places, and that, as the excavation is carried forward, it may not come upon one of these fissures, and so water may flow in.

To my mind there is the most satisfactory answer to be given to such an objection as this. The grey chalk we find is practically watertight, the fresh water which is in the chalk being in the white chalk above the grey; this water finds its way into the sea, as we know, at the level of the beach. Moreover there are fissures undoubtedly in the white chalk, and it is extremely probable that through some of such fissures the fresh water is flowing upwards into the sea, the fresh water having come from a greater height than the sea-level, and having therefore a preponderating force, or head, which would enable it to flow out against the pressure of the sea, and in that way such fissures in the white chalk would be kept open notwithstanding they might be at the bottom of the sea, and in that way also, if in the white chalk a well were sunk close to the sea and powerful pumps were put down, drawing more than was supplied to them by the fresh water coming from inland, these pumps would, in all probability, very soon begin to draw in salt water, which would come in the reverse direction to the fresh water, and therefore from the sea through the fissure which had been kept open by the previously outgoing fresh water. But, as I have said, the grey chalk is practically watertight, and the proof of it is not only that which is seen in the trial heading, but farther in the fact that it upholds the water in the white chalk, and that that water goes out into the sea above the grey chalk. It is well known it is idle to sink a well into the grey chalk with the object of obtaining water. This being so, assume that in bygone times, by some agency, I do not know what, a fissure had been made in the grey chalk where it is exposed in the bed of the sea, what would happen to that fissure? no fresh water could flow

out by it, for by the hypothesis, the grey chalk being impermeable, there is no fresh water in it to flow outwards, and no salt water could continue to flow through it inwards, for there is nowhere for it to go to. There must be therefore an entirely stagnant condition within this fissure. It is not suggested that the water of the English Channel is free from sedimentary matter. Granted this state of things, it appears to me to be inevitable that in process of time such a fissure, having in it stagnant water, must have been filled up with the depositable matter that is in the sea, and must thereby have become closed and puddled up. I therefore look upon it as practically impossible that there can be any open fissure, which would let down the sea water through the grey chalk. Even, however, assuming there were an open fissure, one thing is quite certain, no destruction of life among those engaged in the work of the heading need ever take place from such a cause, because it is perfectly practicable to keep a trial-hole of about two inches in diameter 10, 15, or 20 feet ahead of the principal excavation, the tool for this hole passing up through the hollow shaft of the machine and being worked by it. The tool itself would also be hollow, and if any water were met with it would show itself by issuing through the small hole in the centre of the tool, and thus ample warning would be given. Further, such a tool would enable the width of the fissure to be ascertained; and I, for one, believe it to be within the power of the engineers of the present day, with the means and appliances they have at hand, to bridge a fissure even under a pressure of some 150 lbs. to the inch, if the fissure were not many feet in width; and if it were, then there is a mode other than bridging, by which such a fissure could be traversed.

I have shown you how it is, that the occurrence of a fissure need not lead to disaster by an unexpected giving way of the heading, and the influx of the sea water. This of course refers to the question of the experimental heading, for when the experimental heading is once through, then we may be said to know all about the nature of the stratification, and all uncertainty, and all danger of surprise are at an end. Perhaps you may think it well, that instead of the mere bare statement that if a fissure did occur it could be dealt with, I should give you some detail upon the subject. I will now do so, and I will deal with it in connection with the 7-foot diameter trial heading.

A fissure must either be wide or narrow. If it be wide it occurs as a wide opening at the bottom of the sea (a thing that I believe could not exist without having been detected by soundings). Under such conditions it would be perfectly practicable to lower material into it at the place where the heading is to be made—for example, neat Portland cement—and thus to close the fissure, making an artificial rock to be cut through, but I presume no one believes that any such fissure as this exists. As you are aware, I do not believe that any unclosed fissure at all exists. But I will come now to the case of a narrow fissure, one that we could not localise at the bottom of the sea sufficiently to deal with it in the way of

filling it up where the trial heading is to pass through, say a fissure 4 or 5 feet wide, and I will take it that we are so far below the surface of the sea that the pressure amounts to 150 lbs. per square inch, or a little under 10 tons to the square foot. The 7-foot heading contains 38 superficial feet area, so that the pressure of the water endways against it would be about 380 tons. A single hydraulic press has been made to give 1000 tons pressure; 380 tons therefore is an every-day load for a single hydraulic press. One 20-inch ram would deal with it at only a ton to the circular inch.

I will now ask you to look at the diagram showing the apparatus by which such a fissure could be passed. You will see, fixed into the walls of the excavation, a ring containing a stuffing-box, or it may be a cup-leather such as that used for an hydraulic press; through this stuffing-box would slide, water-tight, a horizontal iron vessel, or tube, of nearly the full diameter of the bore of the trial heading of 50, 70, or 100 feet long, as might be needed, and provided with cutting tools in front set to the full diameter of that bore. This vessel or tube would be open-ended towards the fissure, but would be close-ended towards the trial heading. It would be made capable of slow revolution, and capable of being pushed forward by hydraulic presses having a large margin of power. Water would then be forced in by a pipe led from the stuffing-box to the space between the outside of the vessel and the inside of the heading, and this water would be pumped at a pressure somewhat greater than that produced by the sea, say if the sea give 150 lbs., then this water should be pumped in at 160 lbs. The result of this arrangement would be that before the fissure was reached, and before the sea pressure came upon the tube, the parts would be subjected to a pressure greater than that of the sea, and therefore all the working parts, instead of being exposed to any shock when the sea pressure came on, would absolutely be thereby relieved to a slight extent. The vessel would be slowly revolved, and by its cutting tools would cut its way forward, the material being washed into the interior of the vessel by the water pumped into the annular space on its outside; and until communication with the fissure was made, the material would be allowed to flow out in a regulated manner just enough to preserve a current. It will be seen that this is nothing more than a horizontal boring crown, such as is commonly used by the Diamond Boring Company, with a current of water at the working-face for continuously washing away the *débris*. Under these circumstances the operation would go on until the vessel had advanced, not only to the fissure, but through the fissure and to the opposite side, and had cut into that opposite side a sufficient distance.

It may be said that when all this was accomplished there would still be left a leakage space round about the end of the vessel as it lay in the cavity itself had made on the opposite side of the fissure, this space being equal to the projection of the cutters from the vessel, but I will show you this could be closed in the readiest manner possible.

In the thickness of the metal of the vessel there would be several grooves, each containing an elastic tube; during the passage of the vessel these would be empty, and would lie below the line of its outer circumference, as shown on the detailed figure, but each tube would have a separate pipe leading through the thickness of the metal of the vessel. When the fissure was passed, water would be pumped along these pipes, swelling out the tubes, so as to press hard against the surface of the excavation. In this way a perfect joint would be ensured, and this being obtained, the water would be let out from the inside of the vessel, it would be ascertained that all was tight, the man-hole doors would then be removed, the block of chalk that was in the vessel would be cut to pieces and carried out, and the result would be that the fissure would be bridged by an iron cylindrical vessel water-tight in the chalk at both ends. Then the end of the vessel would be taken away, and the trial heading would be complete across the fissure. I need hardly say that before these appliances were put to work in the heading there would be a thorough rehearsal in some chalk cliff, or other suitable place, on the surface, provision being made to give the full water-pressure.

If it be objected that this, although a feasible operation with a 7-foot diameter hole, would not be a feasible one with 14 feet diameter, the answer to it is that there is no increase in difficulty whatever; the area becomes four times as great, or, allowing the extra diameter needed for the lining, not quite six times as great, and the pressure to be resisted, instead of being about 400 tons, becomes 2400 tons; but such a pressure as this is, as I have said, perfectly under control.

Assuming, however, the grey chalk to be continuous from side to side, I believe I have been most unnecessarily occupying your time by the details of an apparatus which will never be needed; for I must be allowed to repeat once more that I cannot conceive an open fissure existing in a material where there can have been no flow of water, either out, or in, through the fissure, to keep it open.

I could say much more upon the construction of the tunnel did time admit of it; but I must close this first branch of my subject now, and must ask you to consider with me the second branch—*The Working of a Channel Tunnel.*

Whatever may be the future of the ordinary locomotive, whether it be destined to be improved and to continue, or whether it be destined to be superseded by electricity, or by some new motor utilising the energy stored up in the fuel directly and without the intervention of water, no one can doubt that those who at the present time are responsible for the working of railways would prefer to retain for use in the Channel tunnel the ordinary well-known locomotive, if for no other reason than this—that the existing stock of the railway companies on the two sides of the tunnel would suffice for the haulage of the trains, and that no special preparation need be made. But in the

working of the tunnel, still more than in the making of it, the question of the purity of the air becomes of the first importance. In the making of the tunnel, one is dealing with engineers and others accustomed to a somewhat hard life, who, if they cannot get the best air to breathe, are content to put up with something a little worse, and look upon it as part of their business; but when the tunnel is opened and has to be worked, then one is dealing with the travelling public—people who write letters to the *Times*, and whatever else may happen, the air they breathe, wherever they are, must be of the very best quality, and must be ample in quantity, and therefore it is that in the working of the tunnel, still more than in the making of it, does the question of atmospheric purity become one of paramount importance. This being so, it is well to consider what modes there are available, in addition to the ordinary locomotive, for moving the trains.

Not excluding the ordinary locomotive from the list, we find these modes are as follows:—

1. Ordinary Locomotives.
2. Fireless Locomotives.
3. Fireless Locomotives as improved by Dr. Siemens.
4. Ropes.
5. Electricity.
6. The Pneumatic System, where the head of the train is made as a loose-fitting piston in the tunnel, and the air being drawn out in front, the train is propelled by the pressure of the air at the back.
7. Compressed air.

With respect to No. 1, the ordinary locomotive, this, as you know, is the mode of traction adopted in the Mont Cenis, and in the St. Gothard tunnels. The Channel tunnel, provided with two roads, one for the trains going one way, and the other for the trains going in the reverse direction, would have the advantage over the Mont Cenis and the St. Gothard, that currents uniformly flowing in one and the same direction could be maintained in each roadway, and calculations have been made by Mr. Low showing that with the full allowance for the quantity of coke burnt per mile it would be possible to effectually ventilate this tunnel, which after all is only three times as long as the Mont Cenis and a little more than twice as long as the St. Gothard, and it is possible, that having regard to the extreme convenience of using in the tunnel the same type of engine as will be employed on either side of it, the ordinary locomotive will be adopted. I have been through the Mont Cenis tunnel many times and have never felt the slightest inconvenience.

But however desirable it may be on various grounds that the ordinary locomotive should be used, and however much one is justified in that use by the example of the Mont Cenis and the St. Gothard, where (unlike the plan proposed for the Channel tunnel) there is only one opening for the trains running in both directions, and the currents of air are thereby baffled, it appears to me we shall do well to consider the other modes of train traction that I have enumerated.

The first of these is the fireless locomotive; which, as you probably are aware, is driven by the stored-up energy in the very highly heated water, under considerable pressure, with which a vessel representing a boiler is charged. These locomotives have been used to a slight extent upon tramways, but none yet constructed contain an adequate store of energy for the passage of a Channel tunnel.

As improved by Dr. Siemens, however, it is not by any means clear that they could not suffice for that purpose—his proposition being, that in addition to the energy stored up in the heated water, there should be another store in the form of heated fire-brick disposed as in the regenerator for one of his furnaces, and that, in this way, heat should be communicated to the water during the whole passage of the train.

I am by no means prepared to say that an engine thus fitted could not successfully make the passage of the tunnel, and if it could, the only way in which it would affect the air in it would be, that the escape steam from the engine would issue into the atmosphere of the tunnel, and by its condensation would keep that air moist. It is true this objection might be got over by carrying a considerable weight of cold water in the train to condense the steam, or by a surface condenser cooled by currents of air.

The next mode of moving the trains I have suggested is that of ropes. These, as you know, were worked on the Blackwall Railway for very many years and with a certain amount of success, but if there were separate ropes to each roadway, as there were to the Blackwall Railway, stopping and starting for each train, it would not be possible to have more than one train in each roadway at a time, and I trust the Channel tunnel traffic will be far larger than would be consistent with that state of things. And if on the other hand an endless rope were used, up the one roadway and down the other (each rope of course might be divided into sections driven from engines at each end), then although any number of trains could be running at any one time in each roadway, the whole traffic of the tunnel would be stopped on both roads if one of these ropes were to break. I am inclined, therefore, to think this system, although free from sinning against the purity of the air, should not be accepted.

I now come to the electric mode of moving trains. Such a mode is free from any objection as regards vitiating the air. It neither consumes it, nor does it generate carbonic acid, or carbonic oxide, nor does it deliver steam. Looking at the advance that has been made in the applications of electricity to industry, and among these applications to the instances wherein electricity has been employed for the propulsion of tramcars and railway trains, he would be a bold man who ventured to predict that in the course of the next two or three years electricity may not be thoroughly well established as a propelling agent for trains.

Indeed, there is this Session before Parliament a Bill, which has, I believe, passed the House of Commons, for the establishment of

an electric railway between Charing Cross and the Waterloo Station; and it is therefore by no means improbable that by the (I trust) very few years within which the Channel tunnel will be ready for the reception and working of its trains, electric propulsion may have been so far developed as to render its employment in that tunnel judicious.

The next system I adverted to was the pneumatic, that wherein a train is driven along by a very slight differential air-pressure exerted over the area of a piston loosely fitting the tunnel. This mode of propulsion is one that ensures the most ample ventilation, in fact not merely ample but even unnecessary, that it is to say, it involves the changing of the whole of the air in the tunnel at the passage of every train. It will be found, however, that although admirable for working the traffic of a subterranean Metropolitan line with stations at frequent intervals and with very numerous trains, it would not be a very economical mode of working trains through a tunnel 20 miles long; the skin resistance to the passage of the air even at moderate velocities being so great as to involve the consumption of a large amount of horse-power.

The last of the modes of propulsion that I enumerated, is that wherein there is the employment of compressed air, and, setting aside the electrical mode which, as I have said, may, by the time the tunnel is ready for traffic, be so far developed as to be the most advantageous of all, compressed air is the one that commends itself most to my mind. This system of propulsion is now by no means an experiment. In carrying it out, as probably you are aware, there are employed in the train reservoirs of air compressed to any desired extent by means of compressing engines, and this air, on being allowed to escape from these reservoirs through engines practically of the nature of the ordinary steam engines, drives them, and by them draws the train.

Several modes of employing compressed air have been proposed and have been put to work, among others one by Colonel Beaumont, but the mode with which I am best acquainted, and about which therefore I am the most competent to speak to you, is that which has been in practical work every day for now nearly three years at Nantes. In that town there is a service of tramcars starting every ten minutes from each end of the line, which is $3\frac{7}{8}$ miles long, and traversing the very heart of the town and the busiest part of it, running for a considerable distance along the quay, which is one of the leading business thoroughfares.

In this system the air is compressed to only 30 atmospheres, and is allowed to escape by ingenious, but simple, means at regulated pressures to drive the engine, but prior to passing into the engine it is heated by traversing a vessel containing hot water and steam under pressure, which are charged into the vessel before leaving the depot. The bulk of the cars employed contain their own reservoirs and their own engines, but on holidays, and on busy days, cars in the nature of

locomotives are used, drawing a train behind them, and such cars as these make the outward and homeward journey, or $7\frac{3}{4}$ miles in all, without requiring any replenishment with air. No doubt the reservoirs, to contain a sufficient store of air to propel the train through the whole length of the tunnel, would be heavy, but they would be but little, if at all, heavier than the weight of the tender with its load of water and coals, added to the weight of the boiler with its charge of water, both of which are needed in the working of an ordinary locomotive, but are not wanted when compressed air engines are used. But there is, however, no occasion whatever to carry the whole charge of air needed for the entire journey through the tunnel; there will remain in the completed tunnel the air-pipe that was laid down to drive the perforating machinery. This air-pipe can be always kept charged with air. Valves will be provided at frequent intervals, and thus it will be perfectly possible to re-charge the reservoirs, habitually at an intermediate station, or indeed anywhere (say within an eighth of a mile) on the line, should such re-charging from any accidental cause be needed. The great advantage of compressed-air propulsion, under such circumstances as those which prevail in a tunnel 20 miles long, is that every train which goes through, instead of using and vitiating the air, is in its very progression the cause of thorough and efficient ventilation.

As I have said, working by compressed air is no longer an experiment. It is a mode of propulsion that has been in successful daily use for several years in France; and I trust that before many months are over it will be seen in equally successful daily use in London, and, subject to the development of electrical propulsion, and subject to the great temptation to use the ordinary locomotive, it appears to me, as I have said, that compressed air is that one of all the modes by which a train can be moved which commends itself to one's judgment as the system to be employed for the working of a Channel tunnel.

If this lecture has been found more than usually tedious, I must ask you to attribute it to the fact that I have endeavoured to deal with the subject in an entirely abstract manner, and so as to avoid contentious questions of any kind. I set out with that firm determination, and I trust I have not swerved from it, but I do not like to conclude without saying a word on the means by which the advent through the tunnel of undesirable persons may be prevented, and I will illustrate my meaning by showing how a hitherto unsuggested source of danger might be met, and as this source has as yet not been suggested, it is clear no contention can have arisen on the subject, and I cannot therefore be accused of dealing with contentious matter. This danger I will call *smuggling!* and I will ask you to let me describe a plan by which it appears to me that *smuggling* of any kind could be rendered very difficult, and whereby a custom-house officer could have ample time, and perfect security, to examine the luggage of the passengers, or to (it may be) inquire into the *morale* of the passengers themselves.

No doubt most of you are aware of the way in which points, and switch-locks, and gates at level crossings, are interlocked with the signal apparatus. You know that arrangements are made by which it is impossible, for example, that a signal can be given to invite a train to approach until the gate of a level crossing, which should be closed against the common road, is closed; and conversely, when that gate is closed to the common road and is open to the railway, the same arrangements prevent the moving of the gate during the time there is exhibited a signal inviting the train to approach—in the first case the signal must remain at danger, in the second case the gate must remain closed to the common road. I presume most of you are aware, as I have said, that such an arrangement as this exists. I propose to apply it for the stopping of my *smugglers*, and to do so by the following means.

Let me ask you to imagine that there is to be at the outlet of the tunnel, on the surface, a building of great strength, to prevent the possibility of hardy *smugglers* escaping from it, and with station accommodation of such length as to contain the longest train ever known. In this building the custom house should be situated. At the outer end there would be a strong iron grating, so that while that was closed no *smuggler* could jump out of the train and run out into the open country, and at the hinder end of it, towards the tunnel, there would be a similar strong iron grating, which would prevent any *smuggler* jumping out of the train and endeavouring to run back through the tunnel to France. These strong iron gratings would be connected by the interlocking machinery, which would be concealed deep down in the rock, and would be of such a character that although both gratings might be shut at one time, by no possibility could one grating be opened until the other one was fully closed, and this machinery would be worked, not from within the station at all, but from a place exterior to it, the person working it having a view through the grating of what was going on. The result of such an arrangement would be this, that on a train arriving from the Continent and pulling up at the custom house, having in it *smugglers*, and I will take an extreme case, and say even *full of smugglers*, that train would find a closed grating in front of it. The custom-house officers make their examination; while they are doing so, persons outside the station by the apparatus close the grating in the rear of the train; the train is now between two gratings, and we will assume that the officials inside the station find out that the train is full of *smugglers*, and that thereupon a difference of opinion arises between the *smugglers* and the custom-house officers, and it may be that the passengers being desperate characters intent on *smuggling*, overpower the custom-house officers. But even when they had done that it appears to me they would be, as Shakspeare says, "in a very parlous case," for the persons outside who have the command of the apparatus which works the gates, seeing what is going on, do not use it, but they go to the nearest police station and bring down the police, or, in the assumed case of

the train being *full of smugglers*, it may be they go to the nearest fortress and bring down a regiment or two of soldiers; but in any event it seems to me that the *smugglers* would be very badly off, and if there had been a still more numerous body of smugglers needing a second train, that second train when it arrived near the mouth of the tunnel would find itself barred by the inner gate, and the occupants of it could not afford much aid to their comrades who were already caged in the custom house.

I have said I undertook not to enter into any contentious matter, but nobody has suggested that there is any danger of *smuggling*, and I have thought therefore nobody could be offended at my proposal of a means of putting a stop to it, should any one hereafter suggest, that among the dangers arising from a Channel tunnel is that of the surreptitious introduction of goods.

Such an arrangement as I have described would, it seems to me, be free from any one of the three objections urged against other plans for stopping the advent of undesirable persons. It will be seen that as the apparatus must be worked at the passage of every train, it would escape objection No. 1, which is, that any special arrangement to be employed only on an emergency would be of no service when that emergency arose, because from want of use, it would be sure to be out of order. Further, from the circumstance of this habitual use it would escape objection No. 2, which is, that when the special arrangement was needed on an emergency, even if it were not out of order, it would be practically useless, because the men, not being in the habit of working it, would lose their presence of mind, and would fail to do the right thing at the right time; and lastly, this same habitual employment of the apparatus to every train would get over the third objection, for it would prevent the suggestion being made that any one train was treated with suspicion, while previous trains had been allowed to pass unchallenged, as by the plan I propose all trains would be treated alike and no invidious distinctions would be made.

Moreover, there would be no loss of time, as there would be no delay beyond that which must occur somewhere for custom house purposes.

Bear with me for another minute while I describe to you a very simple but effectual arrangement which might be used as an adjunct to the double-grated station. Let there be made at the end of the tunnel, just before it reached the bottom of the shaft, a turntable, say 100 feet in diameter and 10 or 20 feet below the level of the tunnel; on this turntable place a cylindrical block of concrete, say 35 to 40 feet high, and cased all round in armour plate, and line the tunnel at each side of this circular block with armour-plate. Through the concrete block and its armour-plate let the tunnel be carried, so that, when the turntable with its block of armour-cased concrete stood in one position, the tunnel would be continuous, straight through the concrete block, but, when the turntable was revolved one quarter of a

turn, the opening through it would be transverse to the line of the tunnel, which would at that time be therefore effectually stopped by the armour-plated case of the block.

I know you will say "Why this is nothing more than a stop-cock!" I agree that is all that it is, but it would be a very effectual one, and as it would be turned hydraulically, it could be worked from Dover Castle, if it were thought desirable; and I fancy that any soldiers—I beg pardon, I should have said *smugglers*—who came along the tunnel and found the end closed with a smooth curved wall of armour-plate that could not be moved and, short of battering by a hundred-ton gun, could not be injured, would begin to sorrowfully retrace their steps.

In conclusion, let me thank you for the kind attention with which you have honoured me to night, and let me express my hope, and I trust your hope, that no idle apprehension of increased facilities for *smuggling* to be afforded by a Channel tunnel, nor, as at this last moment of my lecture I am all but tempted to say, any equally idle fear of invasion, will stop, or even delay, the execution of one of the most useful works that even this nineteeth century, prolific as it has been in works of great utility, has seen proposed.

[F. B.]

WEEKLY EVENING MEETING,

Friday, May 26, 1882.

THOMAS BOYCOTT, M.D. F.L.S. Manager, in the Chair.

SIR HENRY S. MAINE, K.C.S.I. F.R.S.

Sacred Laws of the Hindus.

THE speaker began by referring to the introduction of the study of Sanscrit, by Sir William Jones, who, on becoming an Indian Judge, found it needful to consult the Hindu law in the original. The law-books of Manu, which he translated, were by him dated about 1200 B.C., and believed to be the work of one man; it is now considered by Sanscrit scholars to be a modern portion of a long-continued series, and it has even been dated as late as about 1300 A.D. They are in verse. Much more ancient books, the earliest in the form of aphorisms, have been discovered, the work of schools of learned Brahmins during many ages. They are essentially religious and liturgical, teaching what men ought to know and do from life to death—theology and morals. There is a perfect continuity of life which runs in a stream, returning in itself, and the doctrine of the transmigration of souls pervades the whole system; the soul of a vicious man enters the body of one of the lowest animals; that of a holy person may be at once united to God. Punishment for sins was also separately effected in twenty-two hells or purgatories; and men were exhorted to torment themselves in this world, to escape worse hereafter. As time went on, the king, whose office in early times was chiefly to enforce penances, became a judge, the chief of a tribunal, and eventually a code of civil law was constituted. The inheritance of property was intimately connected with ancestor worship. When a Brahmin became old he became a hermit, and his property was divided amongst his sons, whose bounden duty it was at his death to appease his spirit by sacrifices. The earnest desire for sons led often to extraordinary forms of artificial sonship, of which Adoption was only one. The authority of the Brahmins was exorbitant; they enjoyed immunity from the sanctions of the criminal laws, and were believed even to have a certain degree of power over the gods. They were priests, legislators, and rulers. On a full examination, their influence is considered to be rather evil than good; and to this may be attributed the many misfortunes of India. The Hindoo mind now is turning towards Western culture and civilisation, and the future government of India, which has to respect both Western and Eastern principles, is a very delicate problem.

WEEKLY EVENING MEETING,

Friday, June 2nd, 1882.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

H. Heathcote Statham, Esq.

The Intellectual Basis of Music.

[The Author regrets that he cannot supply a summary, finding it impossible to state the arguments and illustrations satisfactorily within a limited space.]

GENERAL MONTHLY MEETING,

Monday, June 5, 1882.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

The following Vice-Presidents for the ensuing year were announced :—

Warren De La Rue, Esq. M.A. D.C.L. F.R.S.
Hon. Sir William R. Grove, M.A. D.C.L. LL.D. F.R.S.
Sir Frederick Pollock, Bart. M.A.
William Spottiswoode, Esq. M.A. D.C.L. Pres. R.S.
George Busk, Esq. F.R.S. Treasurer.
William Bowman, Esq. LL.D. F.R.S. Honorary Secretary.

Mrs. J. Stewart Hodgson,
Thomas Ewing Winslow, Esq. Q.C.

were elected Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Accademia dei Lincei, Reale, Roma—Atti, Serie Terza. Vol. VI. Fasc. 11. 4to. 1882.
Agricultural Society of England, Royal—Journal, Second Series, Vol. XVIII. Part 2. 8vo. 1882.
American Philosophical Society—Proceedings, No. 109. 8vo. 1881.
Antiquaries, Society of—Archæologia, Vol. XLVII. Part 1. 4to. 1882.

Asiatic Society of Bengal—Proceedings, No. 2. 8vo. 1882.
Journal, Extra No. to Part I. 1880. 8vo. 1882.
Descriptions of New Lepidopterous Insects, Part 2. By F. Moore. 4to. Calcutta, 1882.
Astronomical Society, Royal—Monthly Notices, Vol. XLII. No. 6. 8vo. 1882.
British Architects, Royal Institute of—Proceedings, 1881–2, No. 15. 4to.
British Association for the Advancement of Science—Report of Meeting at York, 1881. 8vo. 1882.
Cornwall Polytechnic Society—Forty-ninth Annual Report, 1881. 8vo. 1882.
Chamberlin, T. C. Esq.—Geology of Wisconsin, Vol. III. With Atlas. 1881.
Chemical Society—Journal for May, 1882. 8vo.
East India Association—Journal, Vol. XIV. No. 2. 8vo. 1882.
Editors—American Journal of Science for May, 1882. 8vo.
Analyst for May, 1882. 8vo.
Athenæum for May, 1882. 4to.
Chemical News for May, 1882. 4to.
Engineer for May, 1882. fol.
Horological Journal for May, 1882. 8vo.
Iron for May, 1882. 4to.
Nature for May, 1882. 4to.
Revue Scientifique and Revue Politique et Littéraire for May, 1882. 4to.
Telegraphic Journal for May, 1882. fol.
Franklin Institute—Journal, No. 677. 8vo. 1882.
Geographical Society, Royal—Proceedings, New Series, Vol. IV. No. 6. 8vo. 1882.
Geological Society—Abstracts of Proceedings, 1881–2, Nos. 421, 422. 8vo.
Jablonowski'sche Gesellschaft, Leipzig, Furstliche—Preisschrift, No. 23. 4to. 1882.
Jameson, Rev. E. O. (the Author)—A Memorial of the Rev. William Cogswell, D.D. 8vo. 1881.
Leete, Joseph, Esq. F.S.S.—The Family of Leete. Collected by the late C. Bridger, and edited by J. C. Anderson. 4to. 1881.
Manchester Geological Society—Transactions, Vol. XVI. Parts 14, 15. 8vo. 1882.
Pharmaceutical Society of Great Britain—Journal, May, 1882. 8vo.
Photographic Society—Journal, New Series, Vol. VI. No. 8. 8vo. 1882.
Purdy, Frederick, Esq. F.S.S. M.R.I.—Return of the Poor Rate Valuation. 4to. 1882.
Ramsay, A.—Scientific Roll, May, 1882. 8vo.
Royal Society of London—Proceedings, No. 219. 8vo. 1882.
Society of Arts—Journal, May, 1882. 8vo.
Symons, G. J.—Monthly Meteorological Magazine, May, 1882. 8vo.
United Service Institution, Royal—Journal, Nos. 114, 115. 8vo. 1882.
Upsal University—Bulletin Mensuel de l'Observatoire Météorologique, Vol. XIII. 4to. 1881–2.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1882: No. 4. 4to.
Wilkins, F. C.E. (the Author)—Wrecks and Drowning. (K 105) 8vo. 1882.

WEEKLY EVENING MEETING,

Friday, June 9, 1882.

Sir Frederick Bramwell, F.R.S. in the Chair.

J. Burdon-Sanderson, M.D. LL.D. F.R.S. Jodrell Professor of Physiology in University College.

*The Excitability of Plants.**

Part I. Animal Excitability.

The subject which I have to bring before you this evening may be best defined as relating to one of the essential endowments of protoplasm, i. e. of the living material out of which the forms of animal and plant life are moulded. It is one which we associate rather with the nature of animals than with that of plants, though it is common to both. In every kind of living matter we are able to observe an alternation between quiescence and activity: and that the transition from the former to the latter is determined by external influences, in other words, excited by stimuli. But in general we confine the term *excitability* to those cases in which the transition is sudden and obvious, and particularly to those in which it is attended or followed by visible changes of form of the excited parts.

When, in 1874, I had the honour of giving an address on a subject included in the present, I had to announce what was then a new discovery, namely, that a phenomenon which had been long known to be characteristic of the transition from rest to action in animal protoplasm also manifested itself in the plant. In all animal structures which are excitable or irritable, i. e. which possess the property of being suddenly called into action when excited, it is found that the waking up—the transition from rest to action—is attended by an electrical disturbance which is of short duration, precedes action, and follows excitation.

It is well known that there are parts of plants which show the same kind of "wakeableness"—which pass suddenly from quiescence into motion when stimulated; but until 1873 the question had not been asked whether, here also, the going into action is attended with electrical change?

In consequence of a suggestion made by Mr. Darwin one summer

---

* Preceded by an abstract of a discourse on "Animal Excitability," delivered on February 25, 1881.

morning in that year, that if such were found to be the case it would afford strong confirmation of the view to which he had been led by entirely different considerations, of the close relation which subsists between the essential vital processes of plants and animals, I undertook at once to examine into the subject. It was the rough result of that inquiry that I brought before you in 1874. What we expected to observe was observed. It was found that in the leaf of *Dionœa muscipula*, which was selected as the example of plant-excitability best adapted for the purpose, the touching of the sensitive hairs was immediately followed by an electrical disturbance, which preceded the visible motion of the leaf. As the electrical phenomena observed strikingly resembled those which present themselves under similar conditions in animals, there seemed no room for doubt that the analogy which had suggested the discovery was a true one. But in 1876, Professor Munk, of Berlin, an animal physiologist of the highest reputation, published an elaborate paper on Dionæa, in which, while he admitted that the facts which had been recorded were in the main true, and that a real relation existed between the electrical disturbance which follows excitation in Dionæa and the so-called "negative variation" of animal physiology, he charged me with having entirely misinterpreted and misunderstood that relation; and in 1877 another still more important research was published by Dr. Kunkel, in which the question was approached from the side of plant-physiology. Professor Kunkel's experiments related not to Dionæa but to Mimosa. His conclusions were as directly opposed to those of Dr. Munk as they were to mine; for his main position was that all electromotive phenomena observed in the organs of plants, are dependent on changes in the distribution of water in their tissues, and consequently have nothing whatever in common with the electromotive phenomena of muscle and nerve. Even had there not been other good reasons for resuming the investigation of the subject, this contradiction of opinion would have rendered it necessary.

Every one who contemplates the behaviour of the sensitive plant, or of the Flytrap, is led to exclaim: If it had but nerves we could understand it! Let us for a moment inquire what there is in nerve which these animal-plants seem to require. The question is easily answered. Nerve is the channel by which, in the animal body, the influence of any change which takes place in one part of the organism is conveyed to other parts at a distance, independently of the transmission of any sensible motion. Haller, who is well called the father of physiology, sought to explain the propagation of influence in nerve, from the organ of the will to the muscles which it governs, by the transmission of motion of liquid contained in a tube (as in this little apparatus in which my hand represents the will, the long flexible tube the nerve, and the indicator at its farther end the muscle). It is more than a century since Haller made this comparison, but even then he was behind his time, for a greater than he —Newton—had clearly recognised that the process by which the

brain becomes cognisant of what goes on at the surface of the body cannot be attributed to the communication of any visible or sensible motion. Newton, although at that time no one had ever seen nerve-fibres as we now see them under the microscope, yet described them with perfect truth as "pellucid and uniform hair-like filaments," in which vibratory motion could be propagated.* This conception Haller, who was certainly not wanting in imagination, rejected. Failing to understand that the thrills which Newton contemplated were of an order far more subtile than those of sound, he argued that, if the function of nerve were dependent on the propagation of vibratory motion, these would so interfere one with another, that all distinctness of impression and of action would be lost, &c. Haller's doctrine of the nerve-fluid held undisputed sway for a century; we have still traces of it in the language used by medical writers. But the notions which we now entertain on nerve-function are much more allied to those of Newton—so like, indeed, that they might be clothed in his language.

The transmission of an impression, i. e. of a state of excitation in a nerve, has been justly compared to the propagation of a mechanical disturbance along a row of card houses, so arranged that the collapse of any one of them necessarily determines that of its neighbours. Such a structure exhibits the properties assigned by Newton to the pellucid capillamenta of nerves, its card houses corresponding to the particles of which the pellucid substance was conceived by him to be made up. In the one case, as in the other, a disturbance (excitation) which originates at any point is propagated in either direction, reaching its goal in a time which is proportional to the distance travelled.

A still better illustration is furnished by the propagation of an explosion. Here, for example, is a train of gun-cotton.† When I excite the end of the nerve with a match, a blaze runs along to the opposite end, of which you can easily trace the progress, and if, in repeating the experiment, I partially block the explosion by compressing the strand midway by a weight, you see plainly enough that propagation is retarded at the obstacle.

The main ground for the statement I have made to you, that the transmission of an impression along a nerve is analogous to the propagation of an explosion, lies in the proof first given by Helmholtz, that time is lost in the transmission of excitatory effects along nerves, and that the time is proportional to the distance. This I will endeavour to illustrate by an experiment. Tracing the motor-nerve channels by which, in the human body, the influence of the will is conveyed to the muscles of the thumb and finger as in the act of

* See Query 24 at the end of the third Book of Newton's Optics, Dr. Horsley's edition, 1782, p. 226.

† Mr. Abel, with the greatest kindness, enabled me to illustrate this part of the lecture by experiments, showing how, according to the nature of the explosive substance, the velocity of propagation is very different, though the mode of propagation is the same.

pinching, descriptive anatomy teaches me that these approach the surface sufficiently closely to be within reach of induction currents led through at the skin, at two places, namely, above the collar-bone and at the bend of the elbow. If, by the means I have indicated, I excite the nerve at either of these points, the hand involuntarily pinches, but if I measure the time which elapses in the two cases between the excitation and the muscular response, I find it to be different—the difference being the measure of the time occupied by the excitatory change, from the clavicle to the bend of the arm.*

The experiment you have seen not only serves to illustrate the resemblance between the propagation of the excitatory state and that of an explosion, but also to exhibit the contrast between them. It is common to all excitable structures, whether of plant or animal, that, provided the intervals are not too short, the excitations can be repeated any number of times without losing their effect, a fact which can only be explained on the hypothesis that in such structures provision exists for the immediate recuperation of lost energy ; or, in other words, that the machinulæ which take part in the propagation of the excitatory disturbance are endowed with the faculty of quickly recovering their original condition, so as to be ready for another excitation.†

In bringing before you these elementary facts relating to animal excitability, my object is to use them in the comparison we shall shortly have to make between animals and plants in respect of this property. From the last experiment we have learnt, in addition to the fact which it was specially intended to illustrate, that the sudden change of form of a muscle which we call its contraction is of such a nature that the structure shortens in one direction only, and that it gains in thickness in the same proportion that it diminishes in length. The experiment further illustrates the fact that a muscle does not contract of itself, but that it undergoes this change only when it is directly or mediately excited. Bearing these facts in mind, I would ask your attention to some further characteristics of

---

* The measurement is effected by recording the muscular action on a blackened glass plate, which is so fixed to a pendulum that its surface is parallel with the plane of oscillation. The pendulum is allowed to make a single swing from right to left, and in doing so strikes a trigger, the effect of which is to excite the nerve by an induction shock at one or other of the points indicated. Two experiments were made at the lecture in immediate succession, in one of which the nerve was excited at the more distant, in the other at the nearer point. The difference of time between the two records, calculated from the distance from each other of the two tracings, was about $\frac{1}{50}$ of a second. As the distance was about 13 inches, this gives about 66 metres per second as the rate of transmission. This result was probably not far from the truth, but the reader will understand that a measurement made under the conditions of a lecture experiment could not be relied on.

† The extreme shortness of the interval of time between successive excitations of muscle was illustrated by an experiment in which the rheoscopic limb of a frog was kept in repeated spasmodic action by the voice of the lecturer acting on a telephone of which the wires were in contact with the nerve.

the excitatory process in muscle, a knowledge of which is essential to our purpose.

The first of these is, that in every such process the visible response (in the case of muscle the contraction) is separated in time from its cause, the excitation, by a period during which no visible change occurs, although, for reasons which I need not here insist on, molecular changes must be in progress. With suitable appliances it would not have been difficult to prove this to you experimentally in respect of ordinary muscle, but it can be much more easily demonstrated if I substitute for ordinary voluntary muscle, the muscular tissue of the heart, in which the process is about fifteen times as slow. Here, although the excitatory changes occur in the same order, and are of the same nature as in common muscle, the interval between excitation and response, amounting to about a sixth of a second, is very easily perceived.*

It is obvious that this interval may be regarded as a period of transition from the quiescent to the active state, and I told you at the beginning of the lecture that it was always accompanied by electrical changes of a characteristic kind in the excited part. I wish now to show you, that in the muscular substance of the ventricle of the heart, in which we have been able to observe the existence of an interval of apparent inactivity between excitation and visible response, this transition-time is occupied in the way that has been stated. For this purpose we have arranged the ventricle of the heart of a frog in such a way that it can be projected on the screen. At the same time the surface of the ventricle is led off to the galvanometer, the electrodes being applied one at the base the other at the apex. The galvanometer is so arranged that the image is thrown on the screen close to the lever. On exciting the heart as near as possible to the apex, the image shoots off in a direction which indicates that the excited part of the surface of the ventricle becomes negative to the rest, and it is seen at the same time with perfect distinctness that the electrical effect precedes the mechanical, i.e. the rise of the lever.

There are two other facts which are of importance for our purpose, and for the demonstration of which the muscular tissue of the ventricle of the heart of the frog is also available. The first is that during a certain period after each excitation, which M. Marey has called the "refractory period," but which is more correctly termed the period of diminished excitability, the tissue does not respond to a second excitation: the second is, that the duration of the excitatory effect (as indicated by that of the electrical disturbance of the

* For this purpose the conical ventricle of a frog's heart was projected on the screen with a weight attached to its apex, the base being fixed. It was excited directly by an induction shock, an electro-magnetic indicator, interpolated in the primary circuit of the inductorium being also projected. The interval of time between the two events, viz. the induction shock and the response, was made obvious.

mechanical effect which follows it, and of the state of suspended excitability) is governed by the temperature at which the observation is made. The first of these propositions may be illustrated by arranging an experiment in such a manner that a ventricle at 10° C. receives two excitations (induction shocks) at an interval of about a second. Both are effectual, but, if the interval is in the slightest degree shortened, the second fails, for it falls within the period of suspended excitability. The proof of the second proposition can of course only be obtained by series of measurements of the time occupied respectively by the electrical disturbance and by the contraction, at different temperatures; but when successive observations are taken of the same ventricle at temperatures which differ by several degrees, the contrast is very readily appreciated.*

The experiments you have seen this evening may, I trust, have served to illustrate the main facts of animal excitability sufficiently to enable us to proceed to the subject which more specially interests us—that of the excitatory phenomena of plants.

Part II.

The number of plants which exhibit what is often called irritability is very considerable. I will not weary you with even enumerating them. You will see from the table that they are distributed among a number of natural orders, so that one might be inclined to suppose that in this respect no relation could be traced between the physiological endowments and the morphological characters of a plant. That it is not so we have abundant evidence. Thus, in the same genus we may find all the species excitable, though not in the same degree. The extreme sensitiveness of the Chinese Oxalis, formerly called *Biophytum sensitivum*, because it was supposed to be particularly alive, appears in a less degree, but equally distinctly in our own wood-sorrel, as well as in the Tree Oxalis of Bengal—the Carambola,† which is described in an interesting letter addressed by Dr. Robert Bruce to Sir Jos. Banks, and published in the 'Philosophical Transactions.' Again, in the same order, as, for example, among composite plants, we may have the Thistles, Knapweeds, and Hawkweeds, all showing excito-contractility in the same way, although the plants do not at all resemble each other in external appearance. In order to make you

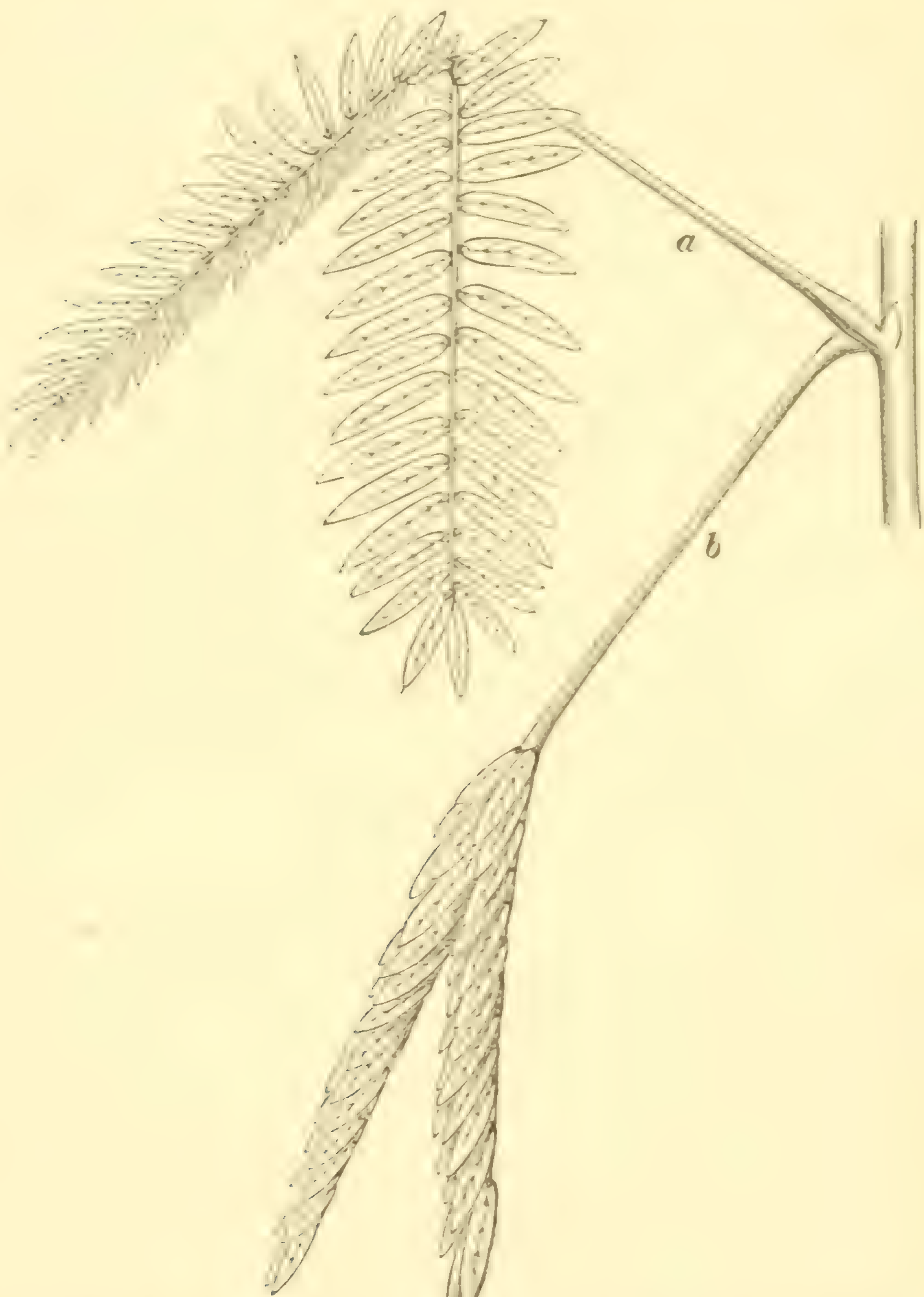
* To illustrate the influence of temperature two ventricles were projected on the screen, of which one was in contact with a lacquered metal surface at 10° C. the other at 15° C. In the latter case the time occupied in the contraction was about half a second shorter than in the former. As regards the period of suspended excitability, it was first shown that at 10° C. the second of two excitations was ineffectual, but by raising the temperature two or three degrees, the state of things was so changed that both excitations were followed by a contraction, the refractory period, like that of systole, being shortened by the warming.

† "An account of the Sensitive Quality of the tree Averrhoa Carambola." By Robert Bruce, M.D. ('Phil. Trans.,' vol. lxxv. p. 356.)

acquainted with the mechanism by which the excitable motions of plants are brought about, I will confine myself to a very few examples, selecting, of course, those which have been most carefully investigated.

Every one is acquainted with the general aspect of the sensitive plant. Probably, also, most persons have observed the way in which the leaves behave when one of them is touched, namely, that the leaf, instead of being directed upwards, suddenly falls, as if it had lost its

FIG. 1.



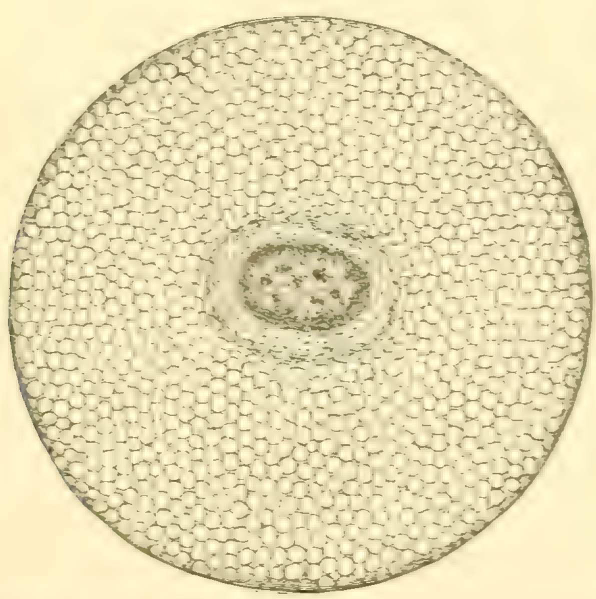
Leaf of Mimosa: *a*, in the unexcited state; *b*, after excitation (after Pfeffer).

power of supporting itself, and that the little leaflets which spring from the side-stalks fold together upwards (Fig. 1). But perhaps every one has not observed exactly how this motion is accomplished, namely, that by means of little cylindrical organs the leaflets are

jointed on to side-stalks, the side-stalk on to the principal stalk, and the principal stalk on to the stem. In those little cylinders, the powers of motion of the leaf have their seat. They may, therefore, be called the motor organs of Mimosa. I would ask your attention to their structure.

In my description I will confine myself to the relatively large joint at the base of the principal leaf-stalk. If you make a section through it in the direction of its length, you find that it consists of the following parts. In the axis of the cylinder is a fibro-vascular bundle; above it are numerous layers of roundish cells with thick walls, and between these there exist everywhere intercellular spaces, which in the resting—that is the excitable—state of the organ, are filled with air. The surface is covered by epidermis. Below the axial bundle there are equally numerous layers of cells, but they differ from the others in this respect, that their walls are more delicate (Fig. 2). And now let us study the mechanism of the motion. The literature

Fig. 2.



Section of the motor organ as projected on the screen. The vascular bundle in the middle of the section consists of a cylinder of thick-walled woody fibres and vessels, surrounded by a layer (annular in section) of elongated cells. The parenchyma is thicker below than above the vascular bundle. The section fails to show that the cells of the upper half have thicker walls.

of this subject is voluminous. Substantially, however, we owe the knowledge we possess to two observers—E. Brücke,* who studied it in 1848, and Pfeffer,† whose work appeared in 1873. I must content myself with the most rapid summary.

Let me begin by noticing that Mimosa, in common with many other excitable plants, exhibits that remarkable phenomenon which

---

* Brücke, "Ueber die Bewegung der Mimosa pudica." Müller's 'Archiv,' 1848, p. 434.

† Pfeffer, 'Physiologische Untersuchungen,' p. 9.

we commonly call the sleep of plants, that is, that as night approaches the leaf-stalks sink, and the leaflets fold up, the whole leaf assuming a position closely resembling that which it assumes when it is irritated. All that time will allow me to say on this subject is that although the leaf assumes the same position in sleep as after excitation, the two effects are not identical. The state of sleep differs from that in which the plant finds itself after it has been irritated in two particulars. The first is, that in the state of sleep it is still excitable, and responds to stimulation exactly in the same way, although from being already depressed the extent of its motion is diminished; the other is, that in sleep, the joint, although bent downwards, is still more or less resistent and elastic; whereas in the unexcitable (or, what comes to the same thing *excited*) state, all elasticity has disappeared. In a word, in the motor organ of Mimosa, in common with all other excitable structures, the characteristic of the excited state is *limpness*. All the Mimosa plants on the table are in the state of sleep, but are still excitable, for when they are touched they sink to an even lower position than that of sleep, and at the same time become limp. Hence you have, as the result of excitation, two changes, namely (1) the change of position, only to be observed when the plant is awake, and (2) the loss of stiffness, dependent, as we shall see, on a vital change in the protoplasm of the cells, which is also observed when the plant is asleep.

So much for the general nature of the excitatory change. How do we discover what the mechanism is by which this remarkable organ of motion acts? By a mode of experiment which is well known to the physiologist. It may be called the method of ablation. We have here a mechanism which consists of several distinct parts, each, we may presume, having a distinct purpose; and the only method which will enable us to discover what these several purposes are is to observe how each acts alone—or, on the other hand, how the rest act after it has been taken away.

To prove that the motion of the whole leaf is dependent on the motor organ at the base of its stalk, requires no experiment. We see that the leaf descends, the joint bends, while the stalk remains rigid, and we know from its structure that the latter contains no mechanism by which it can act mechanically on the joint, as I act on my wrist by the muscles of my fore-arm.

The question therefore is—What part of the joint is essential? We begin by taking away the upper half, leaving the axial bundle and the lower half, and find that the leaf assumes a higher position than before. When touched, it falls. The function of the upper part, therefore, is merely auxiliary. The essential part is the lower, which in the unexcited state is capable of bearing the weight of the leaf. When it is excited it suddenly becomes weak, and the leaf falls. How does it do this? We will proceed to remove the axial bundle. The cellular cushion expands and lengthens, showing that it is elastic, and has a tendency to spring out when liberated. We have seen that this

resistent cushion consists of cells, that is, of little bladders, each of which is distended with liquid; and its tendency to expand as a whole is due to the tendency to expand of the innumerable cells of which it is made up. In the unmutilated state, these are squeezed into a smaller space than that which they would assume if they were left to themselves; and, consequently, as their expansion is prevented, or curbed on one side, it acts on the opposite side, so as to bend the cylinder in the direction of the restraint.

All of this we can, perhaps, better understand by a model; and it is possible to make one which, not only in form, but in principle, corresponds to the living mechanism it is intended to illustrate. In the model the axial bundle is represented by a strip of leather, the innumerable cells of the excitable cushion by an india-rubber bag. By a pump we are able to fill this cell or cushion more or less with fluid, and thus to vary its tension, and you see that if we increase the tension, the stem rises. By diminution it suddenly falls, just as the Mimosa leaf does when irritated.

We have come then to this point—that the reason why the leaf suddenly sinks on excitation is that the cells undergo a sudden diminution of tension or expansion. But our inquiry is not yet terminated. We have still to ask—How is this loss of tension effected? The answer is, by discharge of water. In the unexcited state all these cells are distended or charged with liquid. Suddenly, when the structure is excited, they let out or discharge that liquid, and it finds its way first into the inter-cellular air spaces, and secondly, out of the motor organ altogether. This we know to be a fact by an experiment of Pfeffer's, which must be regarded as one of the most important relating to the mechanism of plants that was ever made. He observed that if the leaf-stalk is cut off from the motor organ, a drop of fluid appears at the cut surface at the moment that the latter bends downwards on excitation, and that in the experiment described just now, in which the upper part of the motor organ is cut off, there is also, so to speak, a sweating of liquid from the cut surface.

We are, therefore, certain that liquid escapes, but why does it escape? That I shall explain farther on, and will now proceed to two other examples. One is a plant which is a great favourite in London, for it is one which flourishes even in London smoke—Mimulus. For our purpose it is good chiefly because its structure is very simple. It is one of those examples in which excitability is associated with the function of fertilisation, and inasmuch as this is a very transitory purpose, the property itself is transitory. When the cells of the stigmatic surface are touched they discharge their liquid contents, and consequently become limp. The outer layer of the lip is elastic, and tends to bend inwards. Consequently when the inner cells lose their elastic resilience it is able to act, and the lip bends inwards. In another allied plant, *Goldfussia anisophylla* (Fig. 3), which was described forty years ago by the Belgian naturalist Morren, we have the same mechanism. In this plant, as

FIG. 3.

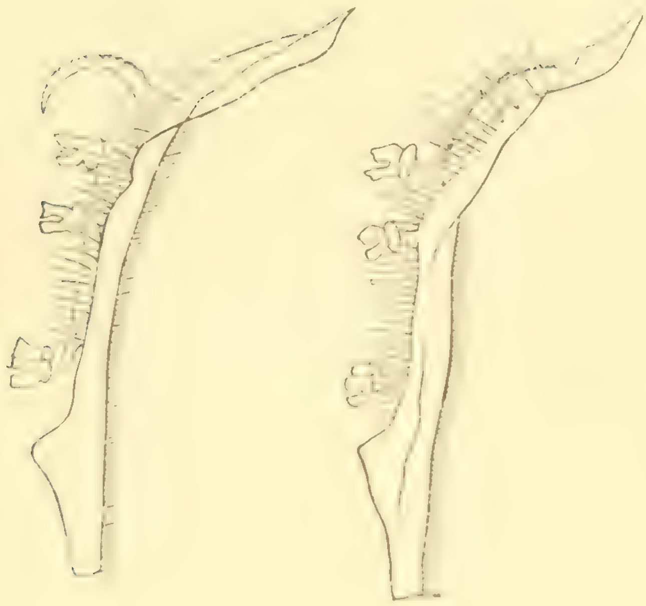


FIG. 4.

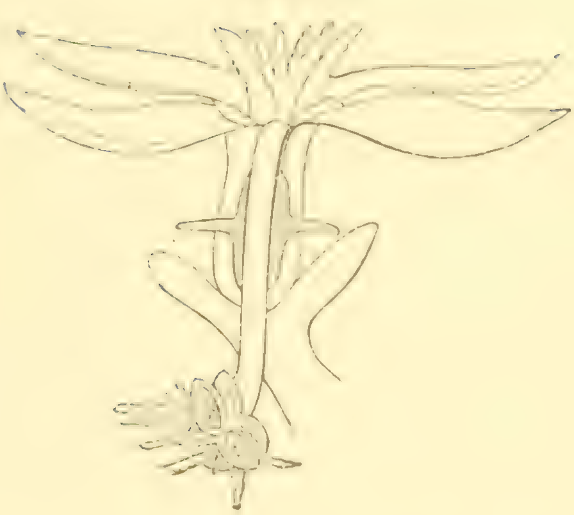


FIG. 5.

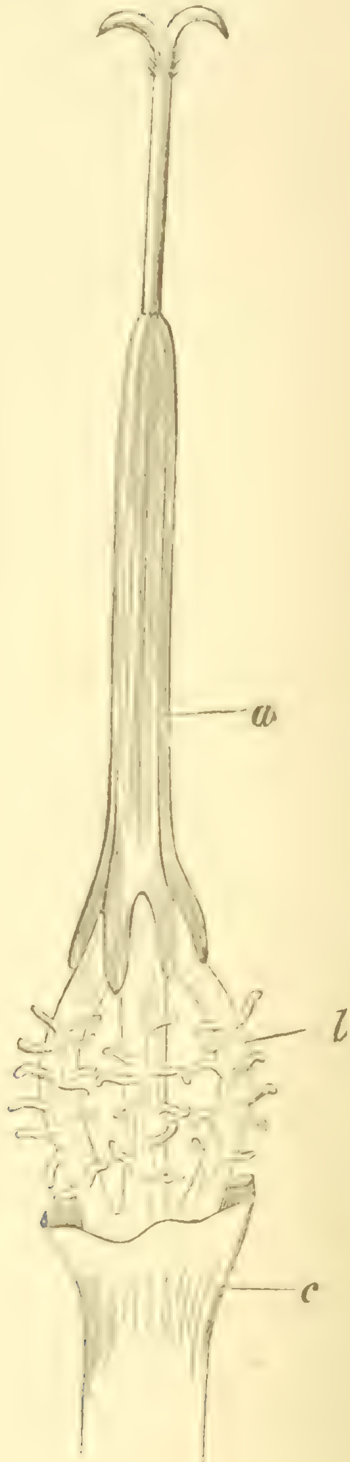


FIG. 3.—Style, stamens, and part of corolla of Goldfussia. In the left-hand figure the style is in the unexcited state, and is curved upwards, so that the stigmatic surface looks towards the mouth of the corolla. On excitation it suddenly assumes the position shown in the second figure, the stigma looking towards the roots of the collecting hairs.

FIG. 4.—Flower of Stylidium, showing the column in the unexcited state, terminating in the anthers and stigma, which are surrounded by conspicuous hairs. It is bent down at the mouth of the corolla, the four principal lobes of which are seen, two on each side, and partly conceals the fifth lobe or labellum.

FIG. 5.—A floret of Centaurea as prepared for projection on the screen. The corolla (*c*) has been cut away so as to expose the five filaments (*b*), beset with hairs, and united above into the anther tube (*a*). The filaments are arched outwards, as in the unexcited state.

shown in the drawing, the style is not lipped but awl-shaped. It reaches to the mouth of the showy, orange-coloured corolla, to the inside of which it is united by its under surface. It has a smooth side, the epidermis of which is made up of numerous small prismatic cells and is very elastic, and, in the unexcited state, concave, and a papillated side beset with the nipple-like ends of cylindrical cells, which, when unexcited, are distended with liquid. These cylindrical cells are continuous with those of the conducting tissue of the style. When an insect enters the flower, it does two things: it charges the fringe of hairs on the inside of the corolla with pollen, and touches the style, which, in consequence, bends suddenly in the opposite direction to that in which it was bent before, so as to plunge its stigmatic surface into the fringe. In this motion the epidermis acts as a spring simply. So long as the stigmatic tissue is turgid it cannot act. The moment its cells lose their tension, off it goes.*

Another plant investigated by Morren is one of very different organisation, but is one in which the existence of excitability has an equally plain teleological interpretation. Long ago Robert Brown, to whom plant-lore owes so much, when exploring the flora of Botany Bay, became acquainted with the now well-known Australian plant called Stylidium.† [A specimen from the Royal Gardens at Kew was exhibited.] Here is the plant (Fig. 4). The flower is too small to be easily seen, but the diagram will enable you to understand the mechanism. It has again to do with insects and fertilisation. In Stylidium the anthers and stigma are united together at the summit of a cylindrical stem which may be compared with the motor organ of Mimulus. You might naturally suppose that they were arranged so in order that the pollen from these anthers should be at once received by the adjoining stigmatic surface. That it is not so is evident from the order of development of the flower, for you find that at the moment that the anthers burst, the stigma is not yet mature. Consequently the pollen is not intended for it, but for flowers which have come to maturity earlier, and the mechanism which now interests us fulfils this purpose. The figure shows the singular form of this strange flower. You observe that the column, as it is called, is bent down over the corolla so as to be in contact with the odd-looking labellum, which here takes the place of one of the petals. At the moment that the anthers burst the column attains its greatest sensitiveness. The slightest touch causes it to spring up, straighten itself suddenly, and then bend over to the opposite side. The mechanism resembles that of Mimosa and of Mimulus. There is a spring, the action of which is restrained by the resilience of cells distended with liquid. Suddenly these cells discharge their contents, and the spring acts.

---

* "Récherches sur le mouvement, &c., du style du Goldfussia anisophylla." Mém. de l'Acad. Royale de Bruxelles, 1839, vol. xii.

† Morren, "Récherches sur le mouvement et l'anatomie du Stylidium graminifolium." Mem. de l'Acad. de Bruxelles, t. xi., 1838.

And now let me pass to another group of plants which may serve as a contrast to Stylidium. Stylidium may be called an out-of-the-way plant. It has an organisation which is not represented in the European flora. The family of thistles, and their allies the knapweeds (represented in our gardens by the ladies' blue-bottle), all of which are common wayside plants, exhibit excitable movements which, although of a very different kind from those we have just described, have, like them, to do with the visits of insects for the purpose of fertilisation. We will now throw on the screen a single fertile floret of *Centaurea Cyanus* (Fig. 5). The large diagram shows the same floret deprived of its corolla. Its axis is occupied by the style, surrounded by its tube of anthers. Below, the anther-filaments expand into a kind of cage, and again approach one another, when they are united with the tube of the corolla. At the moment that the anthers arrive at maturity these filaments are very excitable. When one of them is touched, it contracts and draws the style towards itself. Immediately afterwards the excitatory effect spreads to the others, all five arches becoming straight and applying themselves closely to the style. A similar effect is produced by an induction shock. [The structure described was projected on the screen; on passing an induction current through it, the mode of contraction of the filaments was seen.]

The mechanism of Centaurea has been studied by many plant physiologists, particularly by Professor Ferdinand Cohn of Breslau, and more recently with great completeness by Professor Pfeffer. It has in this respect a greater interest than any other—that the shortening of these filaments in response to excitation strikingly resembles muscular contraction. You have here a structure in the form of a flattened cylinder which resembles many muscles in form, the length of which is diminished by about a sixth on excitation. This superficial resemblance between the two actions makes it the more easy to appreciate the differences.

Let me draw your attention to the diagram of an experiment made last year, which was intended to illustrate the nature of muscular contraction, and particularly to show that when a muscle contracts, it does not diminish in volume. The first difference between muscle and plant is a difference in the degree of shortening. A muscle shortens by something like a third of its length, the anther filament only by a sixth. But it is much more important to notice that in contracting, the filaments do not retain their volume. In shortening, they broaden, but the broadening is scarcely measurable; hence they must necessarily diminish in bulk, and this shrinkage takes place, as Pfeffer has shown, exactly in the same manner as that in which the excitable cushion of Mimosa shrinks, namely, by the discharge of liquid from its cells.

We are now in a position to study more closely the question to which I referred a few minutes ago—How do the cells discharge their contents? The structure of the filament of Centaurea, from its

extreme simplicity, is a better subject of investigation with reference to this question, than any other. Each filament is a ribbon consisting of (1) a single fibro-vascular bundle, (2) delicate cells of regular cylindrical form, (3) an epidermis of somewhat thick-walled cells. [Microscopical preparations were shown.] In Mimosa we saw that the epidermis and vascular bundle took only a passive part in the production of the motion. Here, the part they play is even less important. Everything depends on the parenchyma, which, when excited, shrinks by discharging its water. Pfeffer proved this by cutting off the anther tube from the filaments, and then observing that on excitation a drop collected on the cut surface, which was reabsorbed as the filament again became arched. It is obvious that if the whole parenchyma discharges its liquid, each cell must do the same, for it is made up entirely of cells. To understand how each cell acts, we have only to consider its structure. Each consists of two parts—an external sac or vesicle, which is of cellulose, and, so long as the cell is in the natural or unexcited state, *over-distended*, so that, by virtue of its elasticity, it presses on the contents with considerable force; and secondly, of an internal more actively living membrane of protoplasm, of which the mechanical function is, so long as it is in its active condition, to charge itself fuller and fuller with liquid—the limit to further distension being the elastic envelope in which it is enclosed. In this way the two (the elastic envelope and the protoplasmic lining) are constantly in antagonism, the tendency of the former being towards discharge, that of the latter towards charge. This being so, our explanation of the effect of excitation on the individual cell amounts to this—that the envelope undergoes no change whatever, but that the protoplasm lining suddenly loses its water-absorbing power, so that the elastic force of the envelope at once comes into play and squeezes out the cell-contents. Consequently, although here, as everywhere, the protoplasm is the seat of the primary change, the mechanical agent of the motion is not the protoplasm, but the elastic envelope in which it is enclosed.

The complete knowledge we have gained, from our study of the anther filaments of Centaurea, of the mechanism of the excitable plant-cell, can be applied to every other known example of irrito-contractility in the organs of plants, and particularly to that most remarkable of all such structures, the leaf of *Dionœa muscipula*. Although I described the structure of the leaf just eight years ago in this room, I will occupy a moment in repeating the description. The blade of the leaf is united on to the stalk by a little cylindrical joint. Here are two models, in one of which the leaf is represented in its closed state, in the other in which it is in its unexcited or open state. The leaf is everywhere contractile — that is, excitable by transmission, but not everywhere susceptible of direct excitation—or, in common language, sensitive. It is provided with special organs, of which we do not find the counterpart in any of the plants to which reference has been made, for the reception of external impres-

sions—organs which, from their structure and position, can have no other function.

The action of the leaf to which the plant owes its name and by which it seizes its prey, is, in its general character, too well known to require description. In the shortest possible terms, it is the sudden change of the outer surface of each lobe of the leaf from convex to concave, and at the same time the crossing of the two series of marginal hairs, as the fingers cross when the hands are clasped. What I desire to show with respect to it is, that here also the agents are individual cells—that is, that the individual elements out of which the whole structure is built are the immediate agents in the production of the movement.

A cross-section of the leaf shows the following facts: If the section be made in the direction of the parallel fibro-vascular bundles which run out from the mid-rib nearly at right angles, and happen to include one of those bundles, it is seen that it consists of three parts, viz. of the fibro-vascular bundle in the middle and equidistant from both borders; of the cylindrical cells of the parenchyma on either side, and of an external and internal epidermis. The external epidermis is smooth and glistening, and its cells possess thicker walls than those on the opposite surface.

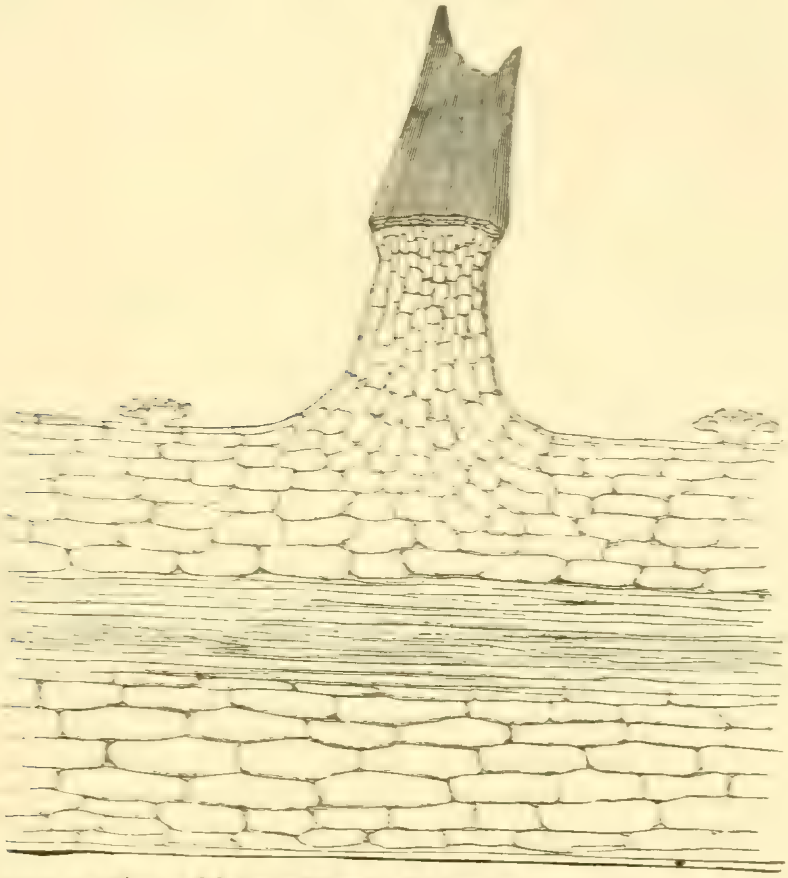
The most remarkable feature of the internal surface is, that it possesses the excitable hairs, three on each side, which in Dionæa are the starting-points of the excitatory process whenever it is stimulated by touch, as is normally the case when the leaf is visited by insects; for experiment shows that although the whole of the leaf can be excited either by pressure or by the passage of an induction current, the hairs exclusively are excited by touch. It is therefore of great interest to know their structure and their relation to the excitable cells of the parenchyma, with which they are in so remarkable a relation physiologically. In sections such as that which we will now project on the screen (Fig. 6), it is seen that each hair springs from a cushion which consists of minute nucleated cells inclosed by epidermis; and that if we follow this structure into the depth of the leaf, its central cells gradually become larger, until they are indistinguishable from those of the ordinary parenchyma of the leaf. By these cells it must be admitted that the endowment of excitability is possessed in a higher degree than by the ordinary cells of the parenchyma, so that for a moment one is tempted to assign to them functions corresponding to those of motor centres in animal structures (particularly in the heart). There is, however, no reason for attributing to them endowments which differ in kind from those we have already assigned to the excitable plant-cell.

The fact that the excitable organs are exclusively on the internal surface of the lobe, suggests that although the parenchyma of the inside has apparently the same structure, it has not the same function as that of the outside—that is, that although the cells of the outer layers are just like those of the inner, they are either not excitable

at all, or are so in a much less degree. In this way only can we account for the bending inwards of the lobe. In the unexcited state both layers are equally turgid; as the effect of excitation the internal layers become limp, the external remaining tense and distended.

I will now endeavour to illustrate the motions of the leaf by projecting them on the screen. Here are several leaves which have been prepared by attaching one of the lobes to a cork support; the other is free, but a very small concave mirror has been attached to

Fig. 6.



Transverse section of lobe of leaf of Dionæa comprising the root of a sensitive hair.

its external surface near the margin. The image of the light which falls on the mirror is reflected on the wall behind me. In this way the slightest movement of the lobe is displayed. By this contrivance I wish to show you two things—first, that a very appreciable time elapses between the excitation and the mechanical effect; and secondly, that when the leaf is subjected to a series of very gentle excitations, the effects accumulate until the leaf closes. This we hope to show by bringing down a camel-hair pencil several times in succession on a sensitive hair, doing it so deftly that at the first touch the lobe will scarcely move at all. At each successive touch it will bend more than at the preceding one, until you see the lever suddenly rise, indicating that the leaf has closed. The purpose which I have in view is to demonstrate the contrast between the motion of the leaf and muscular contraction. A muscle in contracting acts as one

organ—at once. The motion of the leaf is the result of the action of many hundred independent cells, all of which may act together, but may not. In either case they take a great deal longer to think about it; for during a period after excitation, which amounts at ordinary summer temperature to about a second, the leaf remains absolutely motionless.

And now we have to inquire what happens during this period of delay. There are two things which we may assume as certain without further proof, namely, first that something happens—for when I see a certain movement followed after a time invariably by another, I am quite sure that the chain between cause and effect is a continuous one, although the links may be invisible; and secondly, that this invisible change has its seat in the protoplasm of each of the excitable cells.

We have already seen that in muscle this latent state of excitation is not without its concomitant sign—the excitatory electrical disturbance; and I have now to show you that this, which is the sole physical characteristic of the excitatory process in animal tissues, manifests itself with equal constancy and under the same conditions in plants.

It will be unnecessary for my present purpose to enter into any details as to the nature of the electrical change; it will be sufficient to demonstrate with respect to it, first, that when observed under normal physiological conditions, its phenomena are always conformable to certain easily defined characters; secondly, that it culminates before any mechanical effect of excitation is observable, and consequently occupies, for the most part, the period of latent excitation already referred to; and thirdly, that it is transmitted with extreme rapidity from one lobe of the leaf to the opposite. Of these three propositions, it will be convenient to begin with the second. On the left-hand screen is projected the mercurial column of the capillary electrometer of Lippmann. The instrument which we use this evening is one of great sensibility, given me by my friend Professor Lovén, of Stockholm. The capillary electrometer possesses a property which for our purpose is invaluable—that of responding instantaneously to electrical changes of extremely short duration. We cannot better illustrate this than by connecting the wires of the telephone with its terminals. If I press in the telephone-plate I produce an instantaneous difference between the terminals in one direction, and in the opposite when I remove the pressure. You see how beautifully the mercurial column responds.

We now proceed to connect the terminals with the opposite sides of a leaf, so that by means of the mirror we can observe the moment at which the leaf begins to close and the first movement of the mercurial column, both being projected on the same screen. We shall see that the mercurial column responds (so to speak) long before the mirror. The difference of time will be about a second.

We now take another leaf, which, with the plants of which it

forms part, is contained in this little stove, at a temperature of about 32° C. Our object being to subject the leaf to a succession of excitations, the effect of which would of course be to determine its closure, we prevent this by placing a little beam of dry wood across it, and fixing the ends of the beam with plaster of Paris to the marginal hairs of each lobe. At the same time, wedges of plaster are introduced in the gap between the lobes at either end of the mid-rib. [The leaf so fixed was projected on the screen (Fig. 7).] This having been done, we can excite the leaf any number of times without its moving; and we know that we actually excite it by observing the same electrical effect which, in the first leaf experimented on, preceded the movement of the lobe.

Fig. 7.

Dionæa leaf, fixed so as to prevent its closing. (From a photograph.)

Fig. 8.

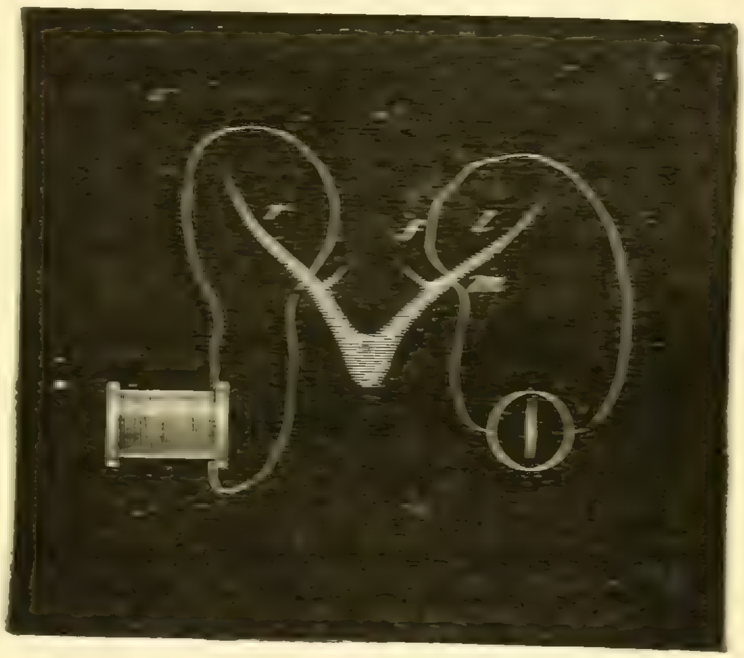


Diagram of ideal transverse section of lamina of leaf of Dionæa. The needle enclosed in a circle represents the electrometer, which, in the experiment described, was substituted for the galvanometer. On the opposite side is shown the secondary coil of the inductorium. *w* is in connection with the capillary, *f* with the sulphuric acid of the electrometer.

And now I beg you to notice what the nature of the experiment is. The diagram (Fig. 8) shows the position of the electrodes by which the opposite surfaces are connected with the terminals of the electrometer. You will notice that they are applied to opposite points of the internal and external surfaces of the right lobe, and

that the left lobe is excited. The experiment consists in this: By the electrodes near *r*, an induction shock passes through the right lobe. Apparently at the same moment the electrometer, which is in relation with the opposite lobe, responds. I say apparently, because in reality we know that the response does not begin until about $\frac{3}{100}$ of a second later. We prove this by a mode of experimentation which is of too delicate a nature to be repeated here. I will explain the mode of action of the instrument used, by a diagram (Fig. 9) which represents a pendulum in the act of swinging from left to

FIG. 9.

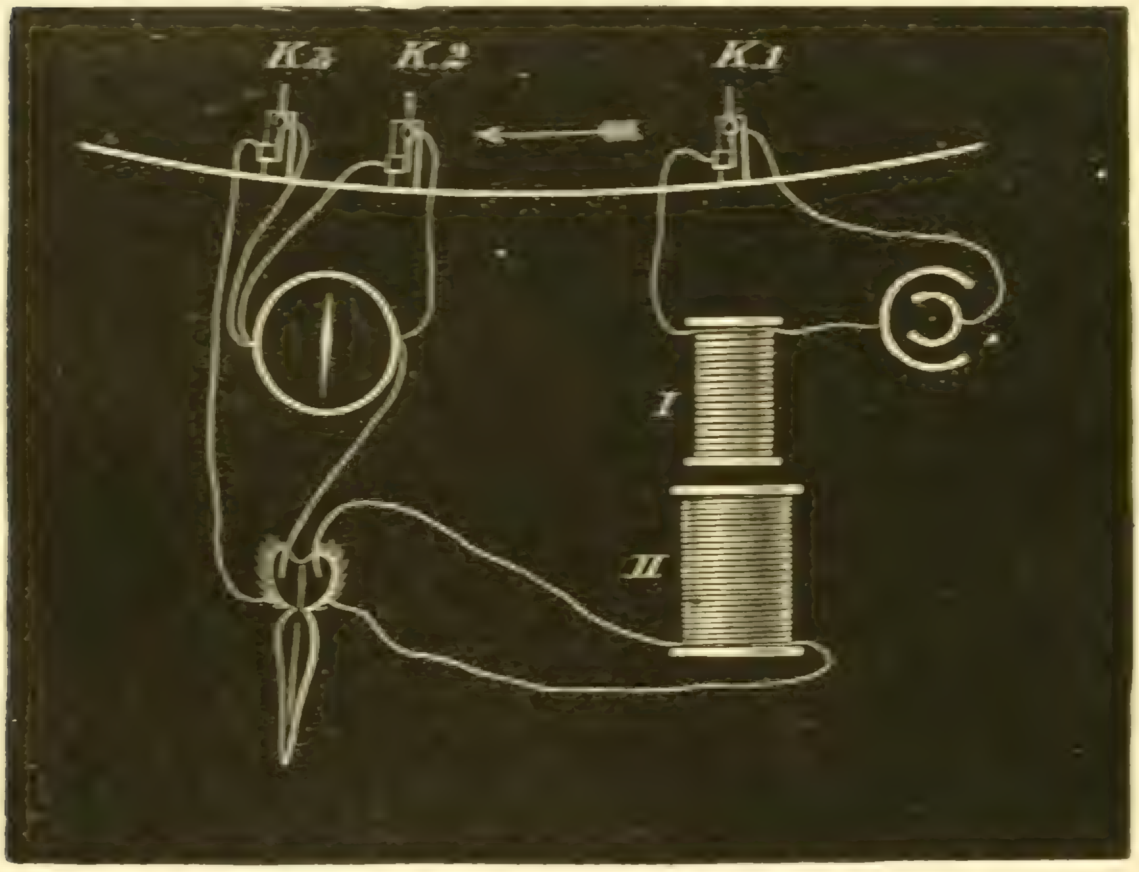


Diagram of the pendulum-rheotome. K_1, K_2, and K_3 are the keys referred to. I. and II. represent respectively the primary and secondary coils of the inductorium. The leaf, galvanometer, battery, &c., will be easily recognised.

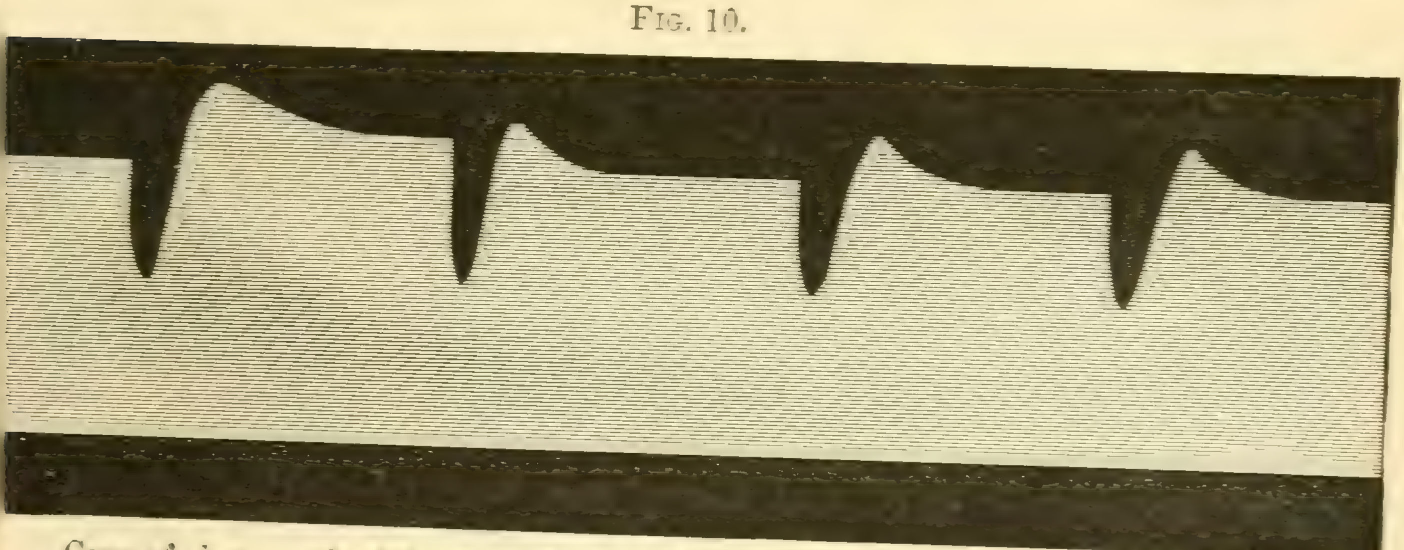
right. As it does so, it opens in succession three keys, of which the first is interpolated in the primary circuit of the induction apparatus which serves to excite the leaf; the second breaks a derivation wire which short-circuits the electrodes, so that, so long as it is closed, no current passes to the galvanometer, which in this experiment takes the place of the electrometer; while the third breaks the galvanometric circuit. Consequently the opposite surfaces of the leaf are in communication with the galvanometer only between the opening of the second and third keys. These three keys can be placed at any desired distance from one another. If they are so placed that the galvanometer circuit is closed $\frac{1}{100}$ of a second after excitation, and opened $\frac{2}{100}$ of a second, and it is found that there is no effect.

it is certain that the electrical disturbance does not begin at the part of the leaf which is interpolated between the galvanometer electrodes until at least $\frac{3}{100}$ of a second after the excitation. If, on extending the period of closure to $\frac{4}{100}$ of a second, the effect becomes observable, you are certain that the disturbance begins between three and four hundredths of a second after excitation.

By this method we have learnt, first, that even when the seat of excitation is as near as possible to the led-off spot, there is a measurable delay, and secondly, that its duration varies with the distance which the excitation effect has to travel so as to indicate that, in a warm stove, the rate of transmission is something like 200 millimeters per second. It is, consequently, comparable with the rate of transmission of the excitatory electrical disturbance in the heart of the frog.

And now I come to my last point, namely that the electrical change has always the same character under the same conditions.

FIG. 10.



Copy of photograph of the excursions of the capillary electrometer as projected on a sensitive plate moving at the rate of $\frac{1}{2}$ centimeter per second. The four "excitatory variations" shown were due to as many touches of a sensitive hair of the lobe opposite to that of which the opposite surfaces were connected with the terminals of the instrument.

You have already seen that when the method used is that which I have indicated, the electrical effect consists of two phases, in the first of which the external surface of the leaf becomes negative to the internal. I will now exhibit this in another way. Many present have probably seen in a recent number of *Nature* reproductions of photographs recently taken by M. Marey, of the phases of the flight of birds. If the movement of a bird's wing can be photographed, you will easily imagine that we can also obtain light-pictures of such a movement as that of the electrometer column. You have only to imagine a sensitive plate moving at a uniformly rapid rate taking the place of the screen, and you have as the result the photograph (Fig. 10) which I show. Here are the electrical effects of several successive excitations recorded by light with unerring exactitude.

In each, the diphasic character is distinct, and you see that the first or negative phase lasts less than a second, but that the positive, of which the extent is much less, is so prolonged that before it has had time to subside it is cut off by another excitation.

It would have been gratifying to me, had it been possible, to exhibit to you other interesting facts relating to the excitatory process in our leaf. It has, I trust, been made clear to you that the *mechanism* of plant motion is entirely different from that of animal motion. But obvious and well marked as this difference is, it is nevertheless not essential, for it depends not on difference of quality between the fundamental chemical processes of plant and animal protoplasm, but merely on difference of rate or intensity. Both in the plant and in the animal, work springs out of the chemical transformation of material, but in the plant the process is relatively so slow that it must necessarily store up energy, not in the form of chemical compounds capable of producing work by their disintegration, but in the mechanical tension of elastic membranes. The plant-cell uses its material *continually* in tightening springs which it has the power of letting off at any required moment by virtue of that wonderful property of excitability which we have been studying this evening. Animal contractile protoplasm, and particularly that of muscle, does work only when required, and in doing so, uses its material directly. That this difference, great as it is, is not essential, we may learn further from the consideration that in those slow motions of the growing parts of plants which form the subject of Mr. Darwin's book, 'On the Movements of Plants,' there is no such storage of energy in tension of elastic membrane, there being plenty of time for the immediate transformation of chemical into mechanical work.

I have now concluded all that I have to say about the way in which plants and animals respond to external influences. In this evening's lecture you have seen exemplified the general fact, applicable alike to the physiology of plant and animal, that whatever knowledge we possess has been gained by experiment. In speaking of Mimosa, I might have entertained you with the ingenious conjectures which were formed as to its mechanism at a time when it was thought that we could arrive at knowledge by reasoning backwards—that is, by inferring from the structure of a living mechanism what its function is likely to be. In certain branches of physiology something has been learnt by this plan, but, as regards our present investigation, almost nothing, nor indeed could anything have been learnt. Everywhere we find that nature's means are adapted to her ends, and the more perfectly the better we know them. But, with rare exceptions, knowledge is got only by actually seeing her at work, for which purpose, if, as constantly happens, she uses concealment, we must tear off the veil, as you have seen this evening, by force. Have we the right to assume this aggressive attitude? Ought we not rather to maintain one of reverent contemplation—waiting till the truth comes to us?

I will not attempt to answer this question, for no thoughtful person ever asked it in earnest. Another question lies behind it, which is a deeper and much older one. Is it worth while? Is the knowledge we seek worth having when we have got it? Notwithstanding that so recently even those who are least conversant with our work have been compelled to acknowledge the beauty and completeness of a life devoted to biological studies, still the question is pressed upon us every hour—How can you think of spending days in striving to unravel the mechanism of a leaf, when you know all the time that if there were no such thing as Dionæa, the world would not be less virtuous or less happy? This question like the other I willingly leave to those who put it. From their point of view it does not admit of an answer; from mine it does not require one. They must go on seeking for and finding virtue and happiness after their fashion: we must go on after ours, striving, by patient continuance in earnest work, to learn year by year some new truth of nature, or to understand some old one better. In so doing, we believe that we also have our reward.

[J. B. S.]

GENERAL MONTHLY MEETING,

Monday, July 3, 1882.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Geological Survey of India: Records. Vol. XV. Part 2. 8vo. 1882.
Accademia dei Lincei, Reale, Roma—Atti. Serie Terza. Vol. VI. Fasc. 12. 4to. 1882.
Asiatic Society of Bengal—Proceedings, No. 3. 8vo. 1882.
Journal, Vol. LI. Part 1, No. 1. 8vo. 1882.
Astronomical Society, Royal—Monthly Notices, Vol. XLII. No. 7. 8vo. 1882.
Australian Museum, Sydney—Catalogue of the Australian Stalk- and Sessile-Eyed Crustacea. By W. A. Haswell. 8vo. Sydney, 1882.
British Architects, Royal Institute of—Proceedings, 1881–2, Nos. 16, 17. 4to.
Chemical Society—Journal for June, 1882. 8vo.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Series II. Vol. II. Part 3. 8vo. 1882.
Douglass, James N. Esq. M.R.I.—The Eddystone Lighthouses (New and Old). By E. P. Edwards and T. Williams. 8vo. 1882.
Editors—American Journal of Science for June, 1882. 8vo.
Analyst for June, 1882. 8vo.
Athenæum for June, 1882. 4to.

Chemical News for June, 1882. 4to.
Engineer for June, 1882. fol.
Horological Journal for June, 1882. 8vo.
Iron for June, 1882. 4to.
Nature for June, 1882. 4to.
Revue Scientifique and Revue Politique et Littéraire for June, 1882. 4to.
Telegraphic Journal for June, 1882. fol.
Franklin Institute—Journal, No. 678. 8vo. 1882.
Geological Institute, Imperial, Vienna—Verhandlungen, Nos. 1-7. 8vo. 1882.
Jahrbuch, Band XXXII. No. 1. 8vo. 1882.
Abhandlungen, Band XII. Heft 3. fol. 1882.
Geological Society—Abstracts of Proceedings, 1881-2, Nos. 423, 424. 8vo.
Quarterly Journal, No. 150. 8vo. 1882.
Gladstone, J. H. Esq. Ph.D. F.R.S. M.R.I. (the Author)—Michael Faraday. (In German.) 16mo. Glogau, 1882.
Haarlem, Société Hollandaise des Sciences—Archives Néerlandaises, Tome XXVII. Liv. 1, 2. 8vo. 1882.
Linnean Society—Journal, Nos. 93, 120. 8vo. 1882.
Meteorological Office—Meteorological Charts for the Ocean District adjacent to the Cape of Good Hope, with Remarks. 4to and fol. 1882.
Meteorological Society—Quarterly Journal, No. 42. 8vo. 1882.
The Meteorological Record, No. 4. 8vo. 1882.
National Association for Social Science—Proceedings, Vol. XV. No. 6. 8vo. 1882.
Numismatic Society—Numismatic Chronicle and Journal. 3rd Series. No. 5. 8vo. 1882.
Owen, Richard, M.D. C.B. F.R.S. (the Author)—Experimental Physiology, its Benefits to Mankind. With an Address on Unveiling the Statue of Wm. Harvey. 12mo. 1882.
Pharmaceutical Society of Great Britain—Journal, June, 1882. 8vo.
Preussische Akademie der Wissenschaften—Sitzungsberichte, I.-XVII. 4to. 1882.
Romanes, George J. Esq. M.A. LL.D. F.R.S. (the Author)—Animal Intelligence. (International Scientific Series, 41.) 16mo. 1882.
Royal Society of London—Philosophical Transactions, Vol. CLXXII. Part 3. 4to. 1882.
Russell, Lord Arthur, M.P. M.R.I.—Report on Electric Lighting Bill. fol. 1882.
Smithsonian Institution, Washington—Annual Report for 1880. 8vo. 1881.
Society of Arts—Journal, June, 1882. 8vo.
Symons, G. J.—Monthly Meteorological Magazine, June, 1882. 8vo.
Telegraph Engineers, Society of—Journal, Vol. XI. No. 42. 8vo. 1882.
Index to Journal, Vols. I.-X. 1872-1882. 8vo. 1882.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1882: No. 5. 4to.
Yorkshire Archæological and Topographical Association—Journal, Part 27. 8vo. 1882.
Zoological Society—Proceedings, 1882, Part 1. 8vo. 1882.
Index to Proceedings, 1871-1880. 8vo. 1882.

GENERAL MONTHLY MEETING.

Monday, November 6, 1882.

HON. SIR WILLIAM R. GROVE, M.A. D.C.L. F.R.S. Vice-President, in the Chair.

Captain W. de W. Abney, R.E. F.R.S.
George Wightwick Rendel, Esq. M.I.C.E.

were elected Members of the Royal Institution.

The special thanks of the Members were given to His Grace THE DUKE OF NORTHUMBERLAND, for his valuable present of 'Descriptive Catalogue of the Antiquities at Alnwick Castle. 1880'; and 'Catalogue of Egyptian Antiquities at Alnwick Castle. 1880.'

The Second Volume of the Classified Catalogue of the Library of the Royal Institution of Great Britain, with Synopsis and Indexes of Authors and Subjects, by Benjamin Vincent, Assistant Secretary and Keeper of the Library, including the Additions from 1857 to 1882, was laid before the Members.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Lords of the Admiralty—Greenwich Observations for 1880. 4to. 1882.
Greenwich Spectroscopic and Photographic Results, 1881. 4to. 1882.
The Governor-General of India—Geological Survey of India: Records. Vol. XV. Part 3. 8vo. 1882.
Board of Trade—Equivalents of Metric and Imperial Weights and Measures. June 1882.
Accademia dei Lincei, Reale, Roma—Atti, Serie Terza. Vol. VI. Fasc. 13, 14. 4to. 1882.
Actuaries, Institute of—Journal. Nos. 126, 127. 8vo. 1882.
American Academy of Arts and Sciences—Memoirs. Vol. XI. Part 1. 4to. 1882.
Antiquaries, Society of—Proceedings. Vol. VIII. No. 4. 8vo. 1882.
Asiatic Society of Bengal—Proceedings, Nos. 4, 5, 6. 8vo. 1882.
Journal. Vol. LI. Part 1, No. 2: Part 2, No. 1. 8vo. 1882.
Asiatic Society, Royal—Journal. Vol. XIV. Part 3. 8vo. 1882.
Astronomical Society, Royal—Monthly Notices, Vol. XLII. No. 8. 8vo. 1882.
Australian Museum, Sydney—Report of the Trustees, 1881. fol. 1882.
Ballard, Robert, Esq. M.I.C.E. (the Author)—Solution of the Pyramid Problem. 8vo. 1882.
Bedford, James, Esq. Ph.D. (the Author)—The Bedfordian System of Astronomy: New Theories of the Universe. 8vo. 1881.
Bombay Education Society's Press—Report on the Census of Berar, 1881. By Eustace J. Kitts. fol. 1882.
British Architects, Royal Institute of—Proceedings, 1881–2, Nos. 18, 19. 1882–3, No. 1. 4to.
Transactions, 1881–2. 4to. 1882.

Buckler, George, Esq. (the Author)—Colchester Castle, Fourth Section. 8vo. 1882.
Chemical Society—Journal for July–Oct. 1882. 8vo.
Civil Engineers' Institution—Minutes of Proceedings, Vols. LXVIII. LXIX. 8vo. 1882.
Clockmakers' Company—Some Account of the Worshipful Company of Clockmakers. By S. E. Atkins and W. H. Overall. 8vo. 1881. (Privately Printed.)
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Series II. Vol. II. Parts 4, 5. 8vo. 1882.
Dax: Société de Borda—Bulletins, 2e Serie, Septième Année: Trimestre 2, 3. 8vo. 1882.
Devonshire Association for the Advancement of Science, Literature, and Art—Report and Transactions, Vol. XIV. 8vo. 1882.
Dialectical Society, London—Quarterly Journal of Transactions, Nos. 1 and 2. 8vo. 1882.
East India Association—Journal, Vol. XIV. No. 3. 8vo. 1882.
Editors—American Journal of Science for July–Oct. 1882. 8vo.
Analyst for July–Oct. 1882. 8vo.
Athenæum for July–Oct. 1882. 4to.
Chemical News for July–Oct. 1882. 4to.
Engineer for July–Oct. 1882. fol.
Horological Journal for July–Oct. 1882. 8vo.
Iron for July–Oct. 1882. 4to.
Nature for July–Oct. 1882. 4to.
Revue Scientifique and Revue Politique et Littéraire for July–Oct. 1882. 4to.
Telegraphic Journal for July–Oct. 1882. fol.
Evans, John, Esq. D.C.L. LL.D. F.R.S. (the Author)—Unwritten History, and how to read it. A Lecture at the British Association, Southampton. 8vo. 1882.
Franklin Institute—Journal, Nos. 679, 680, 681, 682. 8vo. 1882.
Geographical Society, Royal—Proceedings, New Series, Vol. IV. Nos. 7–11. 8vo. 1882.
Supplementary Papers, Vol. I. Part 1. 8vo. 1882.
Geological Society—Quarterly Journal, No. 151. 8vo. 1882.
Glasgow Philosophical Society—Proceedings, Vol. XIII. No. 2. 8vo. 1882.
Iron and Steel Institute—Journal for 1882, Part 1. 8vo. 1882.
Lilienfeld, Paul v. (the Author)—Gedanken über die Socialwissenschaft der Zukunft, Theil 5. 8vo. 1881.
Linnean Society—Transactions, 2nd Ser. Zoology, Vol. II. Part 5. 4to. 1882.
Journal, Nos. 94, 95, 121. 8vo. 1882.
Longmans, Green & Co. (the Publishers)—Longman's Magazine, No. 1. 8vo. 1882.
Madras Literary Society—Madras Journal of Literature and Science for 1881. 8vo. 1882.
Manchester Geological Society—Transactions, Vol. XVI. Parts 16, 17, 18. 8vo. 1882.
Maryland Medical and Chirurgical Faculty—Transactions, 84th Session. 8vo. 1882.
Mechanical Engineers' Institution—Proceedings, No. 2. 8vo. 1882.
Medical and Chirurgical Society—Proceedings, Part 55. 8vo. 1882.
Meteorological Office—Report on Gales in the Ocean District adjacent to the Cape of Good Hope. 4to. 1882.
Communications from the International Polar Commission, Part 3. 4to. St. Petersburg, 1882.
Report on the Storm of October 13–14, 1881. 8vo. 1882.
Meteorological Observations at Stations of the Second Order for 1879. 4to. 1882.
Meteorological Society—Quarterly Journal, No. 43. 8vo. 1882.
The Meteorological Record, No. 5. 8vo. 1882.
Musical Association—Proceedings, 1881–2. 8vo. 1882.
National Association for Social Science—Proceedings, Vol. XV. No. 8. 8vo. 1882.
Norfolk and Norwich Naturalists' Society—Transactions, Vol. III. Part 3. 8vo. 1882.

Northumberland, The Duke of, D.C.L. LL.D. M.R.I.—Descriptive Catalogue of Antiquities at Alnwick Castle. 4to. 1880. (Privately Printed.)
Catalogue of Egyptian Antiquities at Alnwick Castle. By S. Birch. 4to. 1880. (Privately Printed.)
Numismatic Society—Numismatic Chronicle and Journal. 3rd Series. No. 6. 8vo. 1882.
Perry, Rev. S. J. F.R.S. (the Author)—Results of Meteorological and Magnetical Observations, Stonyhurst, 1881. 12mo. 1882.
Pharmaceutical Society of Great Britain—Journal, July–Oct. 1882. 8vo.
Photographic Society—Journal, New Series, Vol. VI. No. 9, and Vol. VII. No. 1. 8vo. 1882.
Physical Society of London—Proceedings, Vol. V. Parts 1, 2. 8vo. 1882.
Preussische Akademie der Wissenschaften—Sitzungsberichte, XVIII.–XXXVIII. 4to. 1882.
Ramsay, A.—Scientific Roll, No. 8. 8vo. 1882.
Royal College of Surgeons of England—Calendar. 8vo. 1882.
Royal Society of Literature—Transactions, Vol. XII. Part 3. 8vo. 1882.
Royal Society of London—Proceedings, Nos. 220, 221. 8vo. 1882.
Society of Arts—Journal, July–Oct. 1882. 8vo.
Squire, Peter, Esq. F.L.S. M.R.I. (the Author)—Companion to the British Pharmacopæia. 13th ed. 8vo. 1882.
Statistical Society—Journal, Vol. XLV. Parts 2, 3. 8vo. 1882.
St. Pétersbourg, Académie des Sciences—Mémoires, 7e Série, Tome XXX. Nos. 3, 5. 4to. 1882.
Bulletin, Tome XXVIII. No. 2. 4to. 1882.
Symons, G. J. Esq. F.R.S. (the Compiler, &c.)—British Rainfall, 1881. 8vo. 1882.
Monthly Meteorological Magazine, July–Oct. 1882. 8vo.
Tasmania Royal Society—Report for 1880. 8vo. 1881.
Telegraph Engineers, Society of—Journal, Vol. XI. No. 43. 8vo. 1882.
Tidy, C. Meymott, Esq. M.B. F.C.S. M.R.I. (the Author)—Legal Medicine. Part 1. 8vo. 1882.
Tokio University—Memoirs, Nos. 6, 7, 8. 4to. 1882.
Twining, Thomas, Esq. M.R.I. (the Author)—Familiar Lessons on Food and Nutrition, Part 1. 16mo. 1882.
Tyndall, John, Esq. D.C.L. F.R.S. M.R.I. (the Author)—Action of Free Molecules on Radiant Heat, and its Conversion thereby into Sound. (Phil. Trans. 1882.) 4to. 1882.
United Service Institution, Royal—Journal, No. 116. 8vo. 1882.
Upsal University—Nova Acta, Ser. III. Vol. XI. Fasc. 1. 4to. 1882.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1882: Nos. 6, 7. 4to.
Victoria Institute—Journal of Transactions, No. 62. 8vo. 1882.
Victoria Public Library, &c. Trustees of the—Catalogue of the Public Library of Victoria. 2 vols. 8vo. 1880.
Wild, Dr. H. (the Director)—Annalen des Physikalischen Central-Observatoriums, 1881, Theil I. 4to. 1882.
Zoological Society—Proceedings, 1882, Parts 2, 3. 8vo. 1882.
Transactions, Vol. XI. Part 7. 4to. 1882.

GENERAL MONTHLY MEETING,

Monday, December 4, 1882.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Acheson George Bartley, M.D. M.A.
David Edward Hughes, Esq. F.R.S.
George Law, Esq. F.R.G.S. F.Z.S.

were elected Members of the Royal Institution.

Five Candidates for Membership were proposed for election.

The following Lecture Arrangements were announced:—

Professor Tyndall, D.C.L. F.R.S. *M.R.I.*—Six Lectures (adapted to a Juvenile Auditory) on Light and the Eye; on Dec. 28 (Thursday), Dec. 30, 1882; Jan. 2, 4, 6, 9, 1883.

William Crawford Williamson, Esq. F.R.S. Professor of Botany, Owens College, Manchester.—Five Lectures on The Primæval Ancestors of Existing Vegetation, and their Bearing upon the Doctrine of Evolution; on Tuesdays, Jan. 16, 23, 30, and Feb. 6, 13.

Robert Stawell Ball, Esq. LL.D. F.R.S. Andrews Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.—Four Lectures on The Supreme Discoveries in Astronomy; on Tuesdays, Feb. 20, 27, and March 6, 13.

Professor Dewar, M.A. F.R.S. *M.R.I.*—Nine Lectures on The Spectroscope and its Applications; on Thursdays, Jan. 18 to March 15.

R. Bosworth Smith, Esq. M.A.—Four Lectures on Episodes in the Life of Lord Lawrence; on Saturdays, Jan. 20, 27, and Feb. 3, 10.

William H. Stone, M.D.—Three Lectures on Singing, Speaking, and Stammering; on Saturdays, Feb. 17, 24, and March 3.

H. Heathcote Statham, Esq.—Two Lectures on Music as a Form of Artistic Expression; on Saturdays, March 10, 17.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Lords of the Admiralty—Nautical Almanac for 1886. 8vo. 1882.
The Governor-General of India—Palæontologia Indica: Series II. Vol. II. Parts 1–3. fol. 1881–2.
Memoirs, Vol. XIX. Part 1. 8vo. 1882.

Agricultural Society of England, Royal—Journal, No. XXXVI. Part 2. 8vo. 1882.
Astronomical Society, Royal—Monthly Notices, Vol. XLII. No. 9. 8vo. 1882.
Blake, J. H. Esq. F.G.S. (*the Author*)—Address to the Norwich Geological Society, on the Conservancy of Rivers, &c. 8vo. 1881.
Board of Trade—Report on Weights and Measures. fol. 1882.
British Architects, Royal Institute of—Proceedings, 1882–3, Nos. 2, 3. 4to.
Buchan, Alexander, Esq. (*the Author*)—Climate of the British Islands. 8vo. 1882.
Meteorology of Rothesay. 8vo. 1882.
Chemical Society—Journal for Nov. 1882. 8vo.
Civil Engineers' Institution—Minutes of Proceedings, Vol. LXX. 8vo. 1882.
Clinical Society—Transactions, Vol. XV. 8vo. 1882.
Dialectical Society, London—Quarterly Journal of Transactions, No. 3. 8vo. 1882.
East India Association—Journal, Vol. XIV. No. 4. 8vo. 1882.
Editors—American Journal of Science for Nov. 1882. 8vo.
Analyst for Nov. 1882. 8vo.
Athenæum for Nov. 1882. 4to.
Chemical News for Nov. 1882. 4to.
Engineer for Nov. 1882. fol.
Horological Journal for Nov. 1882. 8vo.
Iron for Nov. 1882. 4to.
Nature for Nov. 1882. 4to.
Revue Scientifique and Revue Politique et Littéraire, for Nov. 1882. 4to.
Telegraphic Journal for Nov. 1882. fol.
Franklin Institute—Journal, No. 683. 8vo. 1882.
Gassiot, John Peter, Esq. M.R.I.—Proceedings of the London Electrical Society, 1841–2. 8vo. 1843. With Inaugural Address, by C. V. Walker, to the Society of Telegraph Engineers, 1876.
Geological Institute, Imperial, Vienna—Verhandlungen, Nos. 8–11. 8vo. 1882.
Jahrbuch, Band XXXII. Nos. 2, 3. 8vo. 1882.
Abhandlungen, Band X. fol. 1882.
Geological Society—Quarterly Journal, No. 152. 8vo. 1882.
Abstracts of Proceedings, 1882–3, No. 425. 8vo.
Lilienfeld, Paul v. Esq. (*the Author*)—Gedanken über die Socialwissenschaft der Zukunft. Theil 1–4. 8vo. 1873–9.
Lisbon, Sociedade de Geographia—Bulletin, 3e Serie, No. 4. 8vo. 1882.
Manchester Geological Society—Transactions, Vol. XVII. Parts 1, 2. 8vo. 1882.
Mears, T. Lambert, Esq. M.A. LL.D. M.R.I. (*the Author*)—Analysis of M. Ortolan's Institutes of Justinian, including the History of Roman Law. 12mo. 1876.
The Institutes of Gaius and Justinian. 12mo. 1882.
Medical and Chirurgical Society—Transactions, Vol. LXV. 8vo. 1882.
Mensbrugghe, M. G. Van der (*the Author*)—Sur les moyens proposes pour calmer les Vagues de la Mer. 8vo. 1882.
Meteorological Office—Hourly Readings, 1881. Part I. 4to. 1882.
Quarterly Weather Report, 1879. 4to. 1882.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XXXI. 8vo. 1882.
Numismatic Society—Numismatic Chronicle and Journal. 3rd Series. No. 7. 8vo. 1882.
Pharmaceutical Society of Great Britain—Journal, Nov. 1882. 8vo.
Prince, C. L. Esq. (*the Author*)—Illustrated Account by Hevelius of Mounting Telescopes, &c. 8vo. 1882. (Privately Printed.)
Ramsay, A.—Scientific Roll, No. 9. 8vo. 1882.
Royal Society of London—Philosophical Transactions, Vol. CLXXIII. Parts 1, 2. 4to. 1882.
Royal Society of New South Wales—Journal of Proceedings, Vol. XV. 8vo. 1882.
Annual Report of the Department of Mines for 1880. 4to. 1881.
The Minerals of New South Wales. By A. Liversidge. 2nd ed. 4to. 1882.
New South Wales in 1881. By T. Richards. 2nd ed. 8vo. 1882.
Sanitary Institute of Great Britain—Transactions, Vol. III. 8vo. 1882.

Siemens, Bros. and Co. Messrs.—Reproduction de l'Unité de Resistance à Mercure et Relation du Système des Mesures Electriques (Siemens et Halske). 4to. 1882.

Smithsonian Institution—Report of the Bureau of Ethnology, 1879–80. By J. W Powell. 4to. Washington, 1881.

Society of Arts—Journal, Nov. 1882. 8vo.

St. Pétersbourg, Académie des Sciences—Mémoires, 7e Série, Tome XXX. Nos. 4, 6, 7, 8. 4to. 1882.

Symons, G. J.—Monthly Meteorological Magazine, Nov. 1882. 8vo.

Tyndall, John, Esq. D.C.L. F.R.S. M.R.I. (*the Author*)—On Unveiling the Statue of Thomas Carlyle. 8vo. 1882.

United Service Institution, Royal—Journal, No. 117. 8vo. 1882.

Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1882; No. 8. 4to.

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, April 8, 1881.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Professor Tyndall, D.C.L. F.R.S. *M.R.I.*

The Conversion of Radiant Heat into Sound.

The Royal Society has done me the honour of publishing a long series of memoirs on the interaction of radiant heat and gaseous matter. These memoirs did not escape criticism. Distinguished men, among whom the late Professor Magnus and the late Professor Buff may be more specially mentioned, examined my experiments, and arrived at results different from mine. Living workers of merit have also taken up the question, the latest of whom,* while justly recognising the extreme difficulty of the subject, and while verifying, so far as their experiments reach, what I had published regarding dry gases, find that I have fallen into what they consider grave errors in my treatment of vapours.

None of these investigators appear to me to have realised the true strength of my position in its relation to the objects I had in view. Occupied for the most part with details, they have failed to recognise the stringency of my work as a whole, and have not taken into account the independent support rendered by the various parts of the investigation to each other. They thus ignore verifications, both general and special, which are to me of conclusive force. Nevertheless, thinking it due to them and me to submit the questions at issue to a fresh examination, I resumed some time ago the threads of the inquiry. The results shall in due time be communicated to the Royal Society; but meanwhile I would ask permission to bring to the notice of the Fellows a novel mode of testing the relations of radiant heat to gaseous matter, whereby singularly instructive effects have been obtained.

Last year I became acquainted with the ingenious and original experiments of Mr. Graham Bell, wherein musical sounds are obtained through the action of an intermittent beam of light upon solid bodies. From the first I entertained the opinion that these singular sounds were caused by rapid changes of temperature, producing corresponding changes of shape and volume in the bodies impinged

* Lecher and Pernter, 'Philosophical Magazine,' January, 1881; 'Sitzb. der k. Akad. der Wissensch. in Wien,' July, 1880.

upon by the beam. But if this be the case, and if gases and vapours really absorb radiant heat, they ought to produce sounds more intense than those obtainable from solids. I pictured every stroke of the beam responded to by a sudden expansion of the absorbent gas, and concluded that when the pulses thus excited followed each other with sufficient rapidity, a musical note must be the result. It seemed plain, moreover, that by this new method many of my previous results might be brought to an independent test. Highly diathermanous bodies, I reasoned, would produce faint sounds, while highly adiathermanous bodies would produce loud sounds; the strength of the sound being, in a sense, a measure of the absorption. The first experiment made with a view of testing this idea, was executed in the presence of Mr. Graham Bell; * and the result was in exact accordance with what I had foreseen.

The inquiry has been recently extended so as to embrace most of the gases and vapours employed in my former researches. My first source of rays was a Siemens' lamp connected with a dynamo-machine, worked by a gas-engine. A glass lens was used to concentrate the rays, and afterwards two lenses. By the first the rays were rendered parallel, while the second caused them to converge to a point about seven inches distant from the lens. A circle of sheet zinc provided first with radial slits and afterwards with teeth and interspaces cut through it, was mounted vertically on a whirling table, and caused to rotate rapidly across the beam near the focus. The passage of the slits produced the desired intermittence,† while a flask containing the gas or vapour to be examined received the shocks of the beam immediately behind the rotating disk. From the flask a tube of indiarubber, ending in a tapering one of ivory or boxwood, led to the ear, which was thus rendered keenly sensitive to any sound generated within the flask. Compared with the beautiful apparatus of Mr. Graham Bell, the arrangement here described is rude: it is, however, very effective.

With this arrangement the number of sounding gases and vapours was rapidly increased. But I was soon made aware that the glass lenses withdrew from the beam its most effectual rays. The silvered mirrors employed in my previous researches were therefore invoked; and with them, acting sometimes singly and sometimes as conjugate

---

* On November 29: see 'Journal of the Society of Telegraph Engineers,' December 8, 1880.

† When the disk rotates the individual slits disappear, forming a hazy zone through which objects are visible. Throwing by the clean hand, or better still by white paper, the beam back upon the disk, it appears to stand still, the slits forming so many dark rectangles. The reason is obvious, but the experiment is a very beautiful one.

I may add that when I stand with open eyes in the flashing beam, at a definite velocity of recurrence, subjective colours of extraordinary gorgeousness are produced. With slower or quicker rates of rotation the colours disappear. The flashes also produce a giddiness sometimes intense enough to cause me to grasp the table to keep myself erect.

mirrors, the curious and striking results which I have now the honour to submit to the Members were obtained.

Sulphuric ether, formic ether, and acetic ether being placed in bulbous flasks, their vapours were soon diffused in the air above the liquid. On placing these flasks, whose bottoms only were covered by the liquid, behind the rotating disk, so that the intermittent beam passed through the vapour, loud musical tones were in each case obtained. These are known to be the most highly absorbent vapours which my experiments revealed. Chloroform and bisulphide of carbon, on the other hand, are known to be the least absorbent, the latter standing near the head of diathermanous vapours. The sounds extracted from these two substances were usually weak and sometimes barely audible, being more feeble with the bisulphide than with the chloroform. With regard to the vapours of amylene, iodide of ethyl, iodide of methyl and benzol, other things being equal, their power to produce musical tones appeared to be accurately expressed by their ability to absorb radiant heat.

It is the vapour, and not the liquid, that is effective in producing the sounds. Taking, for example, the bottles in which my volatile substances are habitually kept, I permitted the intermittent beam to impinge upon the liquid in each of them. No sound was in any case produced, while the moment the vapour-laden space above an active liquid was traversed by the beam, musical tones made themselves audible.

A rock-salt cell filled entirely with a volatile liquid and subjected to the intermittent beam produced no sound. This cell was circular and closed at the top. Once, while operating with a highly adiathermanous substance, a distinct musical note was heard. On examining the cell, however, a small bubble was found at its top. The bubble was less than a quarter of an inch in diameter, but still sufficient to produce audible sounds. When the cell was completely filled the sounds disappeared.

It is hardly necessary to state that the pitch of the note obtained in each case is determined by the velocity of rotation. It is the same as that produced by blowing against the rotating disk and allowing its slits to act like the perforations of a syren.

Thus, as regards vapours, prevision has been justified by experiment. I now turn to gases. A small flask, after having been heated in the spirit-lamp so as to detach all moisture from its sides, was carefully filled with dried air. Placed in the intermittent beam it yielded a note so feeble as to be heard only with attention. Dry oxygen and hydrogen behaved like dry air. This agrees with my former experiments, which assigned a hardly sensible absorption to these gases. When the dry air was displaced by carbonic acid, the sound was far louder than that obtained from any of the elementary gases. When the carbonic acid was displaced by nitrous oxide the sound was much more forcible still, and when the nitrous oxide was displaced by olefiant gas it gave birth to a musical note which, when

N 2

the beam was in good condition and the bulb well chosen, seemed as loud as that of an ordinary organ-pipe. We have here the exact order in which my former experiments proved these gases to stand as absorbers of radiant heat. The amount of the absorption and the intensity of the sound go hand in hand.

In 1859 I proved gaseous ammonia to be extremely impervious to radiant heat. My interest in its deportment when subjected to this novel test was therefore great. Placing a small quantity of liquid ammonia in one of the flasks, and warming the liquid slightly, the intermittent beam was sent through the space above the liquid. A loud musical note was immediately produced.

In this relation the vapour of water interested me most, and as I could not hope that at ordinary temperatures it existed in sufficient amount to produce audible tones, I heated a small quantity of water in a flask almost up to its boiling-point. Placed in the intermittent beam, I heard—I avow with delight—a powerful musical sound produced by the aqueous vapour.

Small wreaths of haze, produced by the partial condensation of the vapour in the upper and cooler air of the flask, were however visible in this experiment; and it was necessary to prove that this haze was not the cause of the sound. The flask was therefore heated by a spirit-flame beyond the temperature of boiling water. The closest scrutiny by a condensed beam then revealed no trace of cloudiness above the liquid. From the perfectly invisible vapour however the musical sound issued, if anything, more forcible than before. I placed the flask in cold water until its temperature was reduced from about 90° to 10° C., fully expecting that the sound would vanish at this temperature; but notwithstanding the tenuity of the vapour, the sound extracted from it was not only distinct but loud.

Three empty flasks filled with ordinary air were placed in a freezing mixture for a quarter of an hour. On being rapidly transferred to the intermittent beam, sounds much louder than those obtainable from dry air were produced.

Warming these flasks in the flame of a spirit-lamp until all visible humidity had been removed, and afterwards urging dried air through them, on being placed in the intermittent beam the sound in each case was found to have fallen almost to silence.

Sending, by means of a glass tube, a puff of breath from the lungs into a dried flask, the power of emitting sound was immediately restored.

When, instead of breathing into a dry flask, the common air of the laboratory was urged through it, the sounds became immediately intensified. I was by no means prepared for the extraordinary delicacy of this new method of testing the adiathermancy and diathermancy of gases and vapours, and it cannot be otherwise than satisfactory to me to find that particular vapour, whose alleged deportment towards radiant heat has been most strenuously denied, affirming thus audibly its true character.

After what has been stated regarding aqueous vapour we are prepared for the fact that an exceedingly small percentage of any highly adiathermanous gas diffused in air suffices to exalt the sounds. An accidental observation will illustrate this point. A flask was filled with coal gas and held bottom upwards in the intermittent beam. The sounds produced were of a force corresponding to the known absorptive energy of coal-gas. The flask was then placed upright, with its mouth open upon a table, and permitted to remain there for nearly an hour. On being restored to the beam, the sounds produced were far louder than those which could be obtained from common air.*

Transferring a small flask or a test tube from a cold place to the intermittent beam it is sometimes found to be practically silent for a moment, after which the sounds become distinctly audible. This I take to be due to the vaporisation by the calorific beam of the thin film of moisture adherent to the glass.

My previous experiments having satisfied me of the generality of the rule that volatile liquids and their vapours absorb the same rays, I thought it probable that the introduction of a thin layer of its liquid, even in the case of a most energetic vapour, would detach the effective rays, and thus quench the sounds. The experiment was made and the conclusion verified. A layer of water, liquid formic ether, sulphuric ether, or acetic ether one-eighth of an inch in thickness rendered the transmitted beam powerless to produce any musical sound in the vapours. These liquids being transparent to light, the efficient rays which they intercepted must have been those of obscure heat.

A layer of bisulphide of carbon about ten times the thickness of the transparent layers just referred to, and rendered opaque to light by dissolved iodine, was interposed in the path of the intermittent beam. It produced hardly any diminution of the sounds of the more active vapours—a further proof that it is the invisible heat rays, to which the solution of iodine is so eminently transparent, that are here effectual.

Converting one of the small flasks used in the foregoing experiments into a thermometer bulb, and filling it with various gases in succession, it was found that with those gases which yielded a feeble sound, the displacement of a thermometric column associated with the bulb was slow and feeble, while with those gases which yielded loud sounds the displacement was prompt and forcible.

On January 4 I chose for my source of rays a powerful lime-light, which, when sufficient care is taken to prevent the pitting of the cylinder, works with admirable steadiness and without any noise. I also changed my mirror for one of shorter focus, which permitted a

---

* The method here described is, I doubt not, applicable to the detection of extremely small quantities of fire-damp in mines.

nearer approach to the source of rays. Tested with this new reflector the stronger vapours rose remarkably in sounding power.

Improved manipulation was, I considered, sure to extract sounds from rays of much more moderate intensity than those of the lime-light. For this light, therefore, a common candle flame was substituted. Received and thrown back by the mirror, the radiant heat of the candle produced audible tones in all the stronger vapours.

Abandoning the mirror and bringing the candle close to the rotating disk, its direct rays produced audible sounds.

A red-hot coal, taken from the fire and held close to the rotating disk, produced forcible sounds in a flask at the other side.

A red-hot poker, placed in the position previously occupied by the coal, produced strong sounds.

The temperature of the iron was then lowered till its heat just ceased to be visible. The intermittent invisible rays produced audible sounds.

The temperature was gradually lowered, being accompanied by a gradual and continuous diminution of the sound. When it ceased to be audible the temperature of the poker was found to be below that of boiling water.

As might be expected from the foregoing experiments an incandescent platinum spiral, with or without the mirror, produced musical sounds. When the battery power was reduced from ten cells to three the sounds, though enfeebled, were distinct.

My neglect of aqueous vapour had led me for a time astray in 1859, but before publishing my results I had discovered the error. On the present occasion this omnipresent substance had also to be reckoned with. Fourteen flasks of various sizes, with their bottoms covered with a little sulphuric acid, were closed with ordinary corks and permitted to remain in the laboratory from December 23 to January 4. Tested on the latter day with the intermittent beam, half of them emitted feeble sounds, but half were silent. The sounds were undoubtedly due, not to dry air, but to traces of aqueous vapour.

An ordinary bottle containing sulphuric acid for laboratory purposes, being connected with the ear and placed in the intermittent beam, emitted a faint, but distinct, musical sound. This bottle had been opened two or three times during the day, its dryness being thus vitiated by the mixture of a small quantity of common air. A second similar bottle, in which sulphuric acid had stood undisturbed for some days, was placed in the beam: the dry air above the liquid proved absolutely silent.

On the evening of January 7, Professor Dewar handed me four flasks treated in the following manner:—Into one was poured a small quantity of strong sulphuric acid; into another a small quantity of Nordhausen sulphuric acid; in a third were placed some fragments of fused chloride of calcium; while the fourth contained a small quantity of phosphoric anhydride. They were closed with well-

fitting indiarubber stoppers, and permitted to remain undisturbed throughout the night. Tested after twelve hours, each of them emitted a feeble sound, the flask last mentioned being the strongest. Tested again six hours later, the sound had disappeared from three of the flasks, that containing the phosphoric anhydride alone remaining musical.

Breathing into a flask partially filled with sulphuric acid instantly restores the sounding power, which continues for a considerable time. The wetting of the interior surface of the flask with the sulphuric acid always enfeebles, and sometimes destroys, the sound.

A bulb less than a cubic inch in volume, and containing a little water lowered to the temperature of melting ice, produces very distinct sounds. Warming the water in the flame of a spirit-lamp, the sound becomes greatly augmented in strength. At the boiling temperature the sound emitted by this small bulb * is of extraordinary intensity.

These results are in accord with those obtained by me nearly nineteen years ago, both in reference to air and to aqueous vapour. They are in utter disaccord with those obtained by other experimenters, who have ascribed a high absorption to air and none to aqueous vapour.

The action of aqueous vapour being thus revealed, the necessity of thoroughly drying the flasks when testing other substances becomes obvious. The following plan has been found effective:— Each flask is first heated in the flame of a spirit-lamp till every visible trace of internal moisture has disappeared, and it is afterwards raised to a temperature of about 400° C. While the glass is still hot a glass tube is introduced into it, and air freed from carbonic acid by caustic potash, and from aqueous vapour by sulphuric acid, is urged through the flask until it is cool. Connected with the ear-tube, and exposed immediately to the intermittent beam, the attention of the ear, if I may use the term, is converged upon the flask. When the experiment is carefully made, dry air proves as incompetent to produce sound as to absorb radiant heat.

I also tried to extract sounds from perfumes, which I had proved in 1861 to be absorbers of radiant heat. I limit myself here to the vapours of pachouli and cassia, the former exercising a measured absorption of 30, and the latter an absorption of 109. Placed in dried flasks, and slightly warmed, sounds were obtained from both these substances, but the sound of cassia was much louder than that of pachouli.

Many years ago I had proved tetrachloride of carbon to be highly diathermanous. Its sounding power is as feeble as its absorbent power.

In relation to colliery explosions, the deportment of marsh-gas

---

* In such bulbs even bisulphide of carbon vapour may be so nursed as to produce sounds of considerable strength.

was of special interest. Professor Dewar was good enough to furnish me with a pure sample of this gas. The sounds produced by it, when exposed to the intermittent beam, were very powerful.

Chloride of methyl, a liquid which boils at the ordinary temperature of the air, was poured into a small flask, and permitted to displace the air within it. Exposed to the intermittent beam, its sound exceeded in power that of marsh-gas.

The specific gravity of marsh-gas being about half that of air, it might be expected that the flask containing it, when left open and erect, would soon get rid of its contents. This, however, is not the case. After a considerable interval the film of this gas clinging to the interior surface of the flask was able to produce sounds of great intensity.

A small quantity of liquid bromine being poured into a well-dried flask, the brown vapour rapidly diffused itself in the air above the liquid. Placed in the intermittent beam, a somewhat forcible sound was produced. This might seem to militate against my former experiments, which assigned a very low absorptive power to bromine vapour. But my former experiments were conducted with obscure heat; whereas in the present instance I had to deal with the radiation from incandescent lime, whose heat is in part luminous. Now the colour of the bromine vapour proves it to be an energetic absorber of the luminous rays; and to them, when suddenly converted into thermometric heat in the body of the vapour, I thought the sounds might be due.

Between the flask containing the bromine and the rotating disk I therefore placed an empty glass cell: the sounds continued. I then filled the cell with transparent bisulphide of carbon: the sounds still continued. For the transparent bisulphide I then substituted the same liquid saturated with dissolved iodine. This solution cut off the light, while allowing the rays of heat free transmission: the sounds were immediately stilled.

Iodine vaporised by heat in a small flask yielded a forcible sound, which was not sensibly affected by the interposition of transparent bisulphide of carbon, but which was completely quelled by the iodine solution. It might indeed have been foreseen that the rays transmitted by the iodine as a liquid would also be transmitted by its vapour, and thus fail to be converted into sound.*

To complete the argument:—While the flask containing the bromine vapour was sounding in the intermittent beam, a strong solution of alum was interposed between it and the rotating disk. There was no sensible abatement of the sounds with either bromine or iodine vapour.

In these experiments the rays from the lime-light were converged to a point a little beyond the rotating disk. In the next experiment they were rendered parallel by the mirror, and afterwards rendered

* I intentionally use this phraseology.

convergent by a lens of ice. At the focus of the ice-lens the sounds were extracted from both bromine and iodine vapour. Sounds were also produced after the beam had been sent through the alum solution and the ice-lens conjointly.

Several vapours other than those mentioned in this abstract have been examined, and sounds obtained from all of them. The vapours of all compound liquids will, I doubt not, be found sonorous in the intermittent beam. And, as I question whether there is an absolutely diathermanous substance in nature, I think it probable that even the vapours of elementary bodies, including the elementary gases, when more strictly examined, will be found capable of producing sounds.

[J. T.]

WEEKLY EVENING MEETING,

Friday, January 19th, 1883.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

R. Bosworth Smith, Esq. M.A.
Assistant Master of Harrow School.

The Early Life of Lord Lawrence in India.

This Discourse was an introduction to a short course of lectures entitled 'Episodes in the Life of Lord Lawrence.'*

John Lawrence was born in the north of Ireland, March 4, 1811, his family being of Scoto-Irish origin. His father, Alexander, was an ill-requited Indian officer; his mother, a Knox. Among his school-fellows at Foyle College were his brother Henry and Robert Montgomery, his future colleagues in the Punjaub.

Not too well educated, he reluctantly entered the Indian Civil Service in 1829, going to Delhi, his appointment being eventually in Paniput, inhabited by Sikhs and other tribes of a more powerful character than the ordinary Hindoos.

The duties of a collector of the revenue were exceedingly multifarious, embracing the charge of law and police, agriculture, health, roads, bridges, &c. In discharging these duties John Lawrence acted as a wise, just, humane, beneficent despot, throwing himself heartily into his work, deeply sympathising with the people, trusting greatly to his own eyes and hands, and subjecting himself to severe self-discipline. In fact, during the years he acted as revenue collector in

* Mr. R. Bosworth Smith's 'Life of Lord Lawrence,' in two volumes, was published in February 1883.

Paniput, he was in a course of training for his important future work, which so greatly conduced to the preservation of the Indian empire during the mutiny.

In the latter part of the discourse, the speaker related a number of interesting anecdotes illustrative of John Lawrence's physical strength, method of working, escape from imminent death, acuteness in the detection of crime, remembrance of help given to him, tenderness to sufferers, and other characteristic qualities.

After a severe illness, he returned to England in 1840, on furlough.

WEEKLY EVENING MEETING,

Friday, January 26th, 1883.

William Spottiswoode, Esq. M.A. D.C.L. Pres. R.S. Vice-President, in the Chair.

George J. Romanes, Esq. M.A. F.R.S.

Recent Work on Starfishes.

(No Abstract.)

WEEKLY EVENING MEETING,

Friday, February 2, 1883.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Sir William Thomson, LL.D. F.R.S.

The Size of Atoms.

Four lines of argument founded on observation have led to the conclusion that atoms or molecules are not inconceivably, not immeasurably small. I use the words "inconceivably" and "immeasurably" advisedly. That which is measurable is not inconceivable, and therefore the two words put together constitute a tautology. We leave inconceivableness in fact to metaphysicians. Nothing that we can measure is inconceivably large or inconceivably small in physical science. It may be difficult to understand the numbers expressing the magnitude, but whether it be very large or very small there is nothing inconceivable in the nature of the thing because of its greatness or smallness, or in our views and appreciation and numerical expression of the magnitude. The general result of the four lines of reasoning to which I have referred, founded respectively on the undulatory theory of light, on the phenomena of contact electricity, on capillary attraction, and on the kinetic theory of gases, agrees in showing that the atoms or molecules of ordinary matter must be something like the 1-10,000,000th, or from the 1-10,000,000th to the 1-100,000,000th of a centimetre in diameter. I speak somewhat vaguely, and I do so, not inadvertently, when I speak of atoms and molecules. I must ask the chemists to forgive me if I even abuse the words and apply a misnomer occasionally. The chemists do not know what is to be the atom; for instance, whether hydrogen gas is to consist of two pieces of matter in union constituting one molecule, and these molecules flying about; or whether single molecules each indivisible, or at all events undivided in chemical action, constitute the structure. I shall not go into any such questions at all, but merely take the broad view that matter, although we may conceive it to be infinitely divisible, is not infinitely divisible without decomposition. Just as a building of brick may be divided into parts, into a part containing 1000 bricks, and another part containing 2500 bricks, and those parts viewed largely may be said to be similar or homogeneous; but if you divide the matter of a brick building into spaces of nine inches thick, and then think of subdividing it farther, you find you have come to something which is atomic, that is, indivisible without destroying the elements of the structure. The question of

the molecular structure of a building does not necessarily involve the question, Can a brick be divided into parts? and can those parts be divided into much smaller parts? and so on. It used to be a favourite subject for metaphysical argument amongst the schoolmen whether matter is infinitely divisible, or whether *space* is infinitely divisible, which some maintained, whilst others maintained only that *matter* is not infinitely divisible, and demonstrated that there is nothing inconceivable in the infinite subdivision of space. Why, even time was divided into moments (time-atoms!), and the idea of continuity of time was involved in a halo of argument, and metaphysical—I will not say absurdity—but metaphysical word-fencing, which was no doubt very amusing for want of a more instructive subject of study. There is in sober earnest this very important thing to be attended to, however, that in chronometry, as in geometry, we have absolute continuity, and it is simply an inconceivable absurdity to suppose a limit to smallness whether of time or of space. But on the other hand, whether we can divide a piece of glass into pieces smaller than the 1-100,000th of a centimetre in diameter, and so on without breaking it up, and making it cease to have the properties of glass, just as a brick has not the property of a brick wall, is a very practical question, and a question which we are quite disposed to enter upon.

I wish in the beginning to beg you not to run away from the subject by thinking of the exceeding smallness of atoms. Atoms are not so exceedingly small after all. The four lines of argument I have referred to make it perfectly certain that the molecules which constitute the air we breathe are not very much smaller, if smaller at all, than 1-10,000,000th of a centimetre in diameter. I was told by a friend just five minutes ago that if I give you results in centimetres you will not understand me. I do not admit this calumny on the

FIG. 1.

One centimetre. One millimetre.

Royal Institution of Great Britain; no doubt many of you as Englishmen are more familiar with the unhappy British inch; but you all surely understand the centimetre, at all events it was taught till a few years ago in the primary national schools. Look at that diagram (Fig. 1), as I want you all to understand an inch, a centimetre, a millimetre, the 1-10th of a millimetre, and the 1-100th of a millimetre, the 1-1000th of a millimetre, and the 1-1,000,000th of a millimetre. The diagram on the wall represents the metre; below that the yard; next the decimetre, and a circle of a decimetre diameter, the centimetre and a circle of a centimetre, and the millimetre, which is 1-10th of a centimetre, or in round numbers 1-40th of an inch. We will

adhere however to one simple system, for it is only because we are in England that the yard and inch are put before you at all, among the metres and centimetres. You see on the diagram then the metre, the centimetre, the millimetre, with circles of the same diameter. Somebody tells me the millimetre is not there. I cannot see it, but it certainly is there, and a circle whose diameter is a millimetre, both accurately painted in black. I say there is a millimetre, and you cannot see it. And now imagine *there* is 1-10th of a millimetre, and *there* 1-100th of a millimetre and 1-1000th of a millimetre, and *there* is a round atom of oxygen 1-1,000,000th of a millimetre in diameter. You see them all.

Now we must have a practical means of measuring, and optics supply us with it for thousandths of a millimetre. One of our temporary standards of measurement shall be the wave-length of light; but the wave-length is a very indefinite measurement, because there are wave-lengths for different colours of light, visible and invisible, in the ratio of 1 to 16. We have, as it were—borrowing an analogy from sound—four octaves of light that we know of. How far the range in reality extends above and below the range hitherto measured, we cannot even guess in the present state of science. The table before you (Table I.) gives you an idea of magnitudes of length,

TABLE I.—DATA FOR VISIBLE LIGHT.

Line of Spectrum.	Wave-length in Centimetres.	Wave Frequency, or Number of Periods per Second.
A	$7{\cdot}604 \times 10^{-5}$	$395{\cdot}0 \times 10^{12}$
B	$6{\cdot}867$ „	$437{\cdot}3$ „
C	$6{\cdot}562$ „	$457{\cdot}7$ „
D_1	$5{\cdot}895$ „	$509{\cdot}7$ „
D_2	$5{\cdot}889$ „	
E	$5{\cdot}269$ „	$570{\cdot}0$ „
b	$5{\cdot}183$ „	
F	$4{\cdot}861$ „	$617{\cdot}9$ „
G	$4{\cdot}307$ „	$697{\cdot}3$ „
H_1	$3{\cdot}968$ „	$756{\cdot}9$ „
H_2	$3{\cdot}933$ „	763 6 „

and again of small intervals of time. In the column on the left you have the wave-length of light in fractions of a centimetre; the unit in which these numbers to the left is measured is the 1-100,000th (or 10^{-5}) of a centimetre. We have then, of visible light, wave-lengths from $7\frac{1}{2}$ to 4 nearly, or $3{\cdot}9$. You may say then roundly, that for the wave-lengths of visible light, which alone is what is represented on that table, we have wave-lengths of from 4 to 8 on our scale of 1-100,000th of a centimetre. The 8 is invisible radiation a little below the red end of the spectrum. The lowest, marked by Fraun-

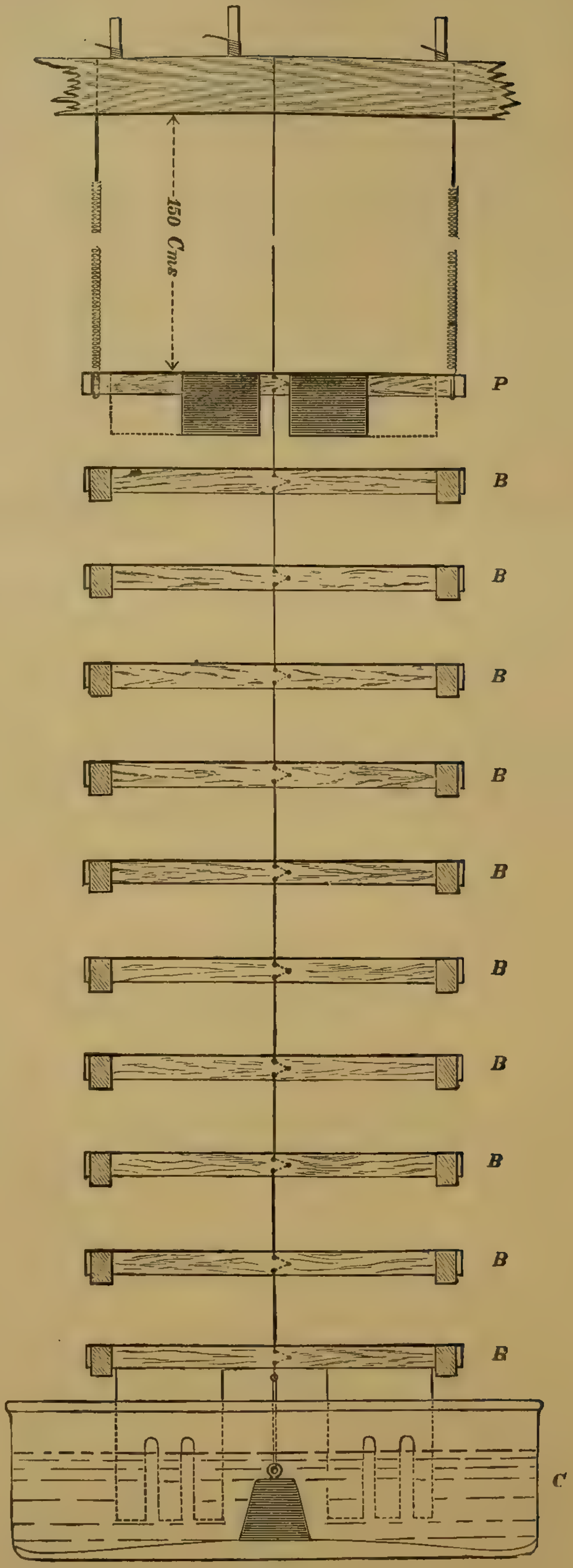
hofer with the letter A, has for wave-length $7\frac{1}{2}$-100,000th of a centimetre. On the model before you I will now show you what is meant by a "wave-length;" it is not length along the crest, such as we sometimes see well marked in a breaking wave of the sea, on a long straight beach ; it is distance from crest to crest of the waves. [This was illustrated by a large number of horizontal rods of wood connected together and suspended bifilarly by two threads in the centre hanging from the ceiling ; * on moving the lowermost rod, a wave was propagated up the series.] Imagine the ends of those rods to represent particles. The rods themselves let us suppose to be invisible, and merely their ends visible, to represent the particles acting upon one another mutually with elastic force, as if of indiarubber bands, or steel spiral springs, or jelly, or elastic material of some kind. They do act on one another in this model through the central mounting. Here again is another model illustrating waves (Fig. 2).† The white circles on the wooden rods represent pieces of matter—I will not say molecules at present, though we shall deal with them as molecules afterwards. Light consists of vibrations transverse to the line of propagation, just as in the models before you.

---

* The details of this bifilar suspension need not be minutely described, as the new form, with a single steel pianoforte wire to give the required mutual forces, described below and represented in Fig. 2, is better and more easily made.

† This apparatus, which is represented in the woodcut, Fig. 2, is of the following dimensions and description. The series of equal and similar bars (B) of which the ends represent molecules of the medium, and the pendulum bar (P), which performs the part of exciter of vibrations, or of kinetic store of vibrational energy, are pieces of wood each 50 centimetres long, 3 centimetres broad, and 1·5 centimetres thick. The suspending wire is steel pianoforte wire No. 22 B. W. G. (·07 of a cm. diameter), and the bars are secured to it in the following manner. Three brass pins of about ·4 of a centimetre diameter are fitted loosely in each bar in the position as indicated ; i.e. forming the corners of an isosceles triangular figure, with its base parallel to the line of the suspending wire, and about 1 mm. to one side of it. The suspending wire, which is laid in grooves cut in the pins, is passed under the upper pin, outside the pin at the apex of the triangle, over the upper side of the lower pin, and thence down to the next bar. The upper end of this wire is secured by being taken through a hole in the supporting beam and several turns of it put round a pin placed on one side of the hole, as indicated in the diagram. To each end of the pendulum bar is made fast a steel spiral spring as shown ; the upper ends of these springs being secured to short cords which pass up through holes in the supporting beam, and are fastened by two or three turns taken round the pins. These steel springs serve as potential stores of vibrational energy alternating in each vibration with the kinetic store constituted by the pendulum bar. The ends of the vibrating bars (B) are loaded with masses of lead attached to them. The much larger masses of lead seen on the pendulum bar, which are adjustable to different positions on the bar, are, in the diagram, shown at the smallest distance apart. The lowermost bar carries two vanes of tin projecting downwards, which dip into viscous liquid (treacle diluted with water) contained in the vessel (c). A heavy weight resting on the bottom of this vessel, and connected to the lower end of the suspending wire by a stretched indiarubber band, serves to keep the lower end of the apparatus in position. The period of vibration of the pendulum bar is adjustable to any desired magnitude by shifting in or out the attached weights, or by tightening or relaxing the cords which pull the upper ends of the spiral springs.

Fig. 2.



To face page 188.

Now in that beautiful experiment well known as Newton's rings we have at once a measure of length in the distance between two pieces of glass to give any particular tint of colour. The wave-length you see, in the distance from crest to crest of the waves travelling up the long model when I commence giving a simple harmonic oscillation to the lowest bar. I have here a convex lens of very long focus, and a piece of plate glass with its back blackened. When I press the piece of glass against the glass blackened behind, I see coloured rings; the phenomenon will be shown to you on the screen by means of the electric light reflected from the space of air between the two pieces of glass. This phenomenon was first observed by Sir Isaac Newton, and was first explained by the undulatory theory of light. [Newton's rings are now shown on the screen before you by reflected electric light.] If I press the glasses together, you see a dark spot in the centre; the rings appear round it, and there is a dark centre with irregularities. Pressure is required to produce that spot. Why? The answer generally given is, because glass repels glass at a distance of two or three wave-lengths of light; say at a distance of 1-5000th of a centimetre. I do not believe that for a moment. The seeming repulsion comes from shreds or particles of dust between them. The black spot in the centre is a place where the distance between them is less than a quarter of a wave-length. Now the wave-length for yellow light is about 1-17,000th of a centimetre. The quarter of 1-17,000th is about 1-70,000th. The place where you see the middle of that black circle corresponds to air at a distance of less than 1-70,000th of a centimetre. Passing from this black spot to the first ring of maximum light, add half a wave-length to the distance, and we can tell what the distance between the two pieces of glass is at this place; add another half wave-length, and we come to the next maximum of light again; but the colour prevents us speaking very definitely because we have a number of different wave-lengths concerned. I will simplify that by reducing it all to one colour, red, by interposing a red glass. You have now one colour, but much less light altogether, because this glass only lets through homogeneous red light, or not much besides. Now look at what you see on the screen, and you have unmistakable evidence of fulcrums of dust between the glass surfaces. When I put on the screw, I whiten the central black spot by causing the elastic glass to pivot, as it were, round the innumerable little fulcrums constituted by the molecules of dust; and the pieces of glass are pressed not against one another, but against these fulcrums. There are innumerable—say thousands—of little particles of dust jammed between the glass, some of them of perhaps 1-3000th of a centimetre in diameter, say 5 or 6 wave-lengths. If you lay one piece of glass on another, you think you are pressing glass on glass, but it is nothing of the kind; it is glass on dust. This is a very beautiful phenomenon, and my first object in showing this experiment was simply because it gives us a linear measure bringing us down at once to 1-100,000th of a centimetre.

Now I am just going to enter a very little into detail regarding the reasons that those four lines of argument give us for assigning a limit to the smallness of the molecules of matter. I shall take contact electricity first, and very briefly. If I take these two pieces of zinc and copper and touch them together at the two corners, they become electrified, and attract one another with a perfectly definite force, of which the magnitude is ascertained from absolute measurements in connection with the well-established doctrine of contact electricity. I do not feel it, because the force is very small. You may do the thing in a measured way; you may place a little metallic knob or projection on one of them of 1-100,000th of a centimeter, and lean the other against it. Let there be three such little metal feet put on the copper; let me touch the zinc plate with one of them, and turn it gradually down till it comes to touch the other two. In this position, with an air-space of 1-100,000th of a centimetre between them, there will be positive and negative electricity on the zinc and copper surfaces respectively, of such quantities as to cause a mutual attraction amounting to 2 grammes weight per square centimetre. The amount of work done by the electric attraction upon the plates while they are being allowed to approach one another with metallic connection between them at the corner first touched, till they come to the distance of 1,100,000th of a centimetre, is 2-100,000ths of a centimetre-gramme, supposing the area of each plate to be one square centimetre.

I will now read you a statement from an article which was published thirteen years ago in 'Nature.' *

"Now let a second plate of zinc be brought by a similar process to the other side of the plate of copper; a second plate of copper to the remote side of this second plate of zinc, and so on till a pile is formed consisting of 50,001 plates of zinc and 50,000 plates of copper, separated by 100,000 spaces, each plate and each space 1-100,000th of a centimetre thick. The whole work done by electric attraction in the formation of this pile is two centimetre-grammes.

"The whole mass of metal is eight grammes. Hence the amount of work is a quarter of a centimetre-gramme per gramme of metal. Now 4030 centimetre-grammes of work, according to Joule's dynamical equivalent of heat, is the amount required to warm a gramme of zinc or copper by one degree Centigrade. Hence the work done by the electric attraction could warm the substance by only 1-16,120th of a degree. But now let the thickness of each piece of metal and of each intervening space be 1-100,000,000th of a centimetre, instead of 1-100,000th. The work would be increased a millionfold unless 1-100,000,000th of a centimetre approaches the smallness of a molecule. The heat equivalent would therefore be enough to

---

* See article "On the Size of Atoms," published in 'Nature,' vol. i. p. 551; printed in Thomson and Tait's 'Natural Philosophy,' second edition, 1883, vol. i. part 2, Appendix F.

raise the temperature of the material by 62°. This is barely, if at all, admissible, according to our present knowledge, or, rather, want of knowledge, regarding the heat of combination of zinc and copper. But suppose the metal plates and intervening spaces to be made yet four times thinner, that is to say, the thickness of each to be 1-400,000,000th of a centimetre. The work and its heat equivalent will be increased sixteenfold. It would therefore be 990 times as much as that required to warm the mass by one degree Centigrade, which is very much more than can possibly be produced by zinc and copper in entering into molecular combination. Were there in reality anything like so much heat of combination as this, a mixture of zinc and copper powders would, if melted in any one spot, run together, generating more than heat enough to melt each throughout; just as a large quantity of gunpowder if ignited in any one spot burns throughout without fresh application of heat. Hence plates of zinc and copper of 1-300,000,000th of a centimetre thick, placed close together alternately, form a near approximation to a chemical combination, if indeed such thin plates could be made without splitting atoms."

In making brass, if we mix zinc and copper together we find no very manifest signs of chemical affinity at all; there is not a great deal of heat developed; the mixture does not become warm, *it does not explode.* Hence we can infer certainly that contact-electricity action ceases, or does not go on increasing according to the same law, when the metals are subdivided to something like 1-100,000,000th of a centimetre. Now this is an exceedingly important argument. I have more decided data as to the actual magnitude of atoms or molecules to bring before you presently, but I have nothing more decided in *giving for certain a limit to supposable smallness.* We cannot reduce zinc and copper beyond a certain thickness without putting them into a condition in which they lose their properties as wholes, and in which, if put together, we should *not* find the same attraction as we should calculate upon from the thicker plates. I think it is impossible, consistently with the knowledge we have of chemical affinities and of the effect of melting zinc and copper together, to admit that a piece of copper or zinc could be divided to a thinness of much less, if at all less, than 1-100,000,000th of a centimetre without separating the atoms or dividing the molecules, or doing away with the composition which constitutes as a whole the solid metal. In short, the structure as it were of bricks, or molecules, or atoms, of which copper and zinc are built up, cannot be much, if at all, less than 1-100,000,000th of a centimetre in diameter, and may be considerably greater.

Similar conclusions result from that curious and most interesting phenomenon, the soap-bubble. Philosophers old and young, who occupy themselves with soap-bubbles, have one of the most interesting subjects of physical science to admire. Blow a soap-bubble and look at it,—you may study all your life perhaps, and still learn lessons in physical science from it. You will now see on the screen the image

of a soap-film in a ring of metal. The light is reflected from the film filling that ring, and focused on the screen. It will show, as you see, colours analogous to those of Newton's rings. As you see the image it is upside down. The liquid streams down (up in the image), and thins away from the highest point of the film. First we see that brilliant green colour. It will become thinner and thinner there, and will pass through beautiful gradations of colour till you see, as now, a deep red, then much lighter, till it becomes a dusky, yellowish-white, then green, and blue, and deep violet, and lastly black, but after you see the black spot it very soon bursts. The film itself seems to begin to lose its tension, when it gets considerably less than a quarter of the wave-length of yellow light, which is the thickness for the dusky white, preceding the final black. When you are washing your hands, you may make and deliberately observe a film like this, in a ring formed by the forefingers and thumbs of two hands, and watch the colours. Whenever you begin to see a black spot or several black spots, the film soon after breaks. The film retains its strength until we come to the black spot, where the thickness is clearly much less than 1-60,000th of a centimetre, which is the thickness of the dusky white.*

Newton, in the following passage in his 'Optics' (pp. 187 and 191 of edition 1721, Second Book, Part I.), tells more of this important phenomenon of the black spot than is known to many of the best of modern observers.

"Obs. 17.—If a bubble be blown with water, first made tenacious by dissolving a little soap in it, it is a common observation that after a while it will appear tinged with a variety of colours. To defend these bubbles from being agitated by the external air (whereby their colours are irregularly moved one among another so that no accurate observation can be made of them), as soon as I had blown any of them I covered it with a clear glass, and by that means its colours emerged in a very regular order, like so many concentric rings encompassing the top of the bubble. And as the bubble grew thinner by the continual subsiding of the water, these rings dilated slowly and overspread the whole bubble, descending in order to the bottom of it, where they vanished successively. In the meanwhile, after all

* Since this lecture was delivered a paper "On the Limiting Thickness of Liquid Films," by Professors Reinold and Rücker, has been communicated to the Royal Society, and an abstract has been published in the 'Proceedings,' No. 225, 1883. The authors give the following results for the thickness of a black film of the liquids specified :—

Liquid.	Method.	Mean Thickness.
Plateau's "Liquide	Electrical.	$\cdot 119 \times 10^{-5}$ cm.
Glycérique."	Optical.	$\cdot 107$,,
Soap Solution.	Electrical.	$\cdot 117$,,
	Optical.	$\cdot 121$,,

The thickness, therefore, of a film of the liquide glycérique and that of a film of a soap solution containing no glycerine are nearly the same, and about 1-50th of the wave-length of sodium light.

the colours were emerged at the top, there grew in the centre of the rings a small round black spot like that in the first observation, which continually dilated itself till it became sometimes more than one-half or three-quarters of an inch in breadth before the bubble broke. At first I thought there had been no light reflected from the water in that place, but observing it more curiously I saw within it several smaller round spots, which appeared much blacker and darker than the rest, whereby I knew that there was some reflection at the other places which were not so dark as those spots. And by farther trial I found that I could see the images of some things (as of a candle or the sun) very faintly reflected, not only from the great black spot, but also from the little darker spots which were within it.

"Obs. 18.—If the water was not very tenacious, the black spots would break forth in the white without any sensible intervention of the blue. And sometimes they would break forth within the precedent yellow, or red, or perhaps within the blue of the second order, before the intermediate colours had time to display themselves."

Now I have a reason, an irrefragable reason, for saying that the film cannot keep up its tensile strength to 1-100,000,000th of a centimetre, and that is, that the work which would be required to stretch the film a little more than that would be enough to drive it into vapour.

The theory of capillary attraction shows that when a bubble—a soap-bubble, for instance—is blown larger and larger, work is done by the stretching of a film which resists extension as if it were an elastic membrane with a constant contractile force. This contractile force is to be reckoned as a certain number of units of force per unit of breadth. Observation of the ascent of water in capillary tubes shows that the contractile force of a thin film of water is about 16 milligrammes weight per millimetre of breadth. Hence the work done in stretching a water film to any degree of thinness, reckoned in millimetre-milligrammes, is equal to sixteen times the number of square millimetres by which the area is augmented, provided the film is not made so thin that there is any sensible diminution of its contractile force. In an article "On the Thermal Effect of Drawing out a Film of Liquid," published in the 'Proceedings' of the Royal Society for April 1858, I have proved from the second law of thermodynamics that about half as much more energy, in the shape of heat, must be given to the film, to prevent it from sinking in temperature while it is being drawn out. Hence the intrinsic energy of a mass of water in the shape of a film kept at constant temperature increases by 24 milligramme-millimetres for every square millimetre added to its area.

Suppose, then, a film to be given with the thickness of a millimetre, and suppose its area to be augmented ten thousand and one fold: the work done per square millimetre of the original film, that is to say per milligramme of the mass, would be 240,000 millimetre-milligrammes. The heat equivalent to this is more than half a degree

Centigrade (0·57°) of elevation of temperature of the substance. The thickness to which the film is reduced on this supposition is very approximately 1-10,000th of a millimetre. The commonest observation on the soap-bubble shows that there is no sensible diminution of contractile force by reduction of the thickness to 1-10,000th of a millimetre; inasmuch as the thickness which gives the first maximum brightness, round the black spot seen where the bubble is thinnest, is only about 1-8000th of a millimetre.

The very moderate amount of work shown in the preceding estimates is quite consistent with this deduction. But suppose now the film to be farther stretched until its thickness is reduced to 1-10,000,000th of a millimetre (1-100,000,000th of a centimetre). The work spent in doing this is two thousand times more than that which we have just calculated. The heat equivalent is 280 times the quantity required to raise the temperature of the liquid by 1° Centigrade. This is far more than we can admit as a possible amount of work done in the extension of a liquid film. It is more than half the amount of work which, if spent on the liquid, would convert it into vapour at ordinary atmospheric pressure. The conclusion is unavoidable, that a water-film falls off greatly in its contractile force before it is reduced to a thickness of 1-10,000,000th of a millimetre. It is scarcely possible, upon any conceivable molecular theory, that there can be any considerable falling off in the contractile force as long as there are several molecules in the thickness. It is therefore probable that there are not several molecules in a thickness of 1-10,000,000th of a millimetre of water.

Now when we are considering the subdivision of matter, look at those beautiful colours which you see in this little casket, left, I believe, by Professor Brande to the Royal Institution. It contains polished steel bars, coloured by having been raised to different degrees of heat, as in the process of annealing hard-tempered steel. These colours, produced by heat on other polished metals besides steel, are due to thin films of transparent oxide, and their tints, as those of the soap-bubble and of the thin space of air in "Newton's rings," depend on the thickness of the film, which, in the case of oxidisable metals, forms, by combination with the oxygen of the air under the influence of heat, a true surface-burning.

You are all familiar with the brilliant and beautifully distributed fringes of heat-colours on polished steel grates and fire-irons escaping that unhappy rule of domestic æsthetics which too often keeps those articles glittering and cold and useless, instead of letting them show the exquisite play of warm colouring naturally and inevitably brought out when they are used in the work which is their reason for existence. The thickness of the film of oxide which gives the first perceptible colour, a very pale orange or buff tint, due to the enfeeblement or extinction of violet light and enfeeblement of blue, and less enfeeblement of the other colours in order, by interference of the reflections from the two surfaces of the film, is about 1-100,000th of

a centimetre, being something less than a quarter wave-length of violet light in the oxide.

The exceedingly searching and detective efficacy of electricity comes to our aid here, and by the force, as it were, spread through such a film, proves to us the existence of the film when it is considerably thinner than that 1-100,000th of a centimetre, when in fact it is so very thin as to produce absolutely no perceptible effect on the reflected light, that is to say, so thin as to be absolutely invisible. If in the apparatus for measuring contact electricity, of which the drawing is before you ('Nature,' vol. xxiii. p. 567), two plates of freshly polished copper be placed in the Volta condenser, a very perfect zero of effect is obtained. If, then, one of the plates be taken out, heated slightly by laying it on a piece of hot iron, and then allowed to cool again and replaced in the Volta condenser, it is found that negative electricity becomes condensed on the surface thus treated, and positive electricity on the bright copper surface facing it, when the two are in metallic connection. If the same process be repeated with somewhat higher temperatures, or somewhat longer times of exposure to it, the electrical difference is augmented. These effects are very sensible before any perceptible tint appears on the copper surface as modified by heat. The effect goes on increasing with higher and higher temperatures of the heating influence, until oxide tints begin to appear, commencing with buff, and going on through a ruddier colour to a dark-blue slate colour, when no farther heating seems to augment the effect. The greatest contact-electricity effect which I thus obtained between a bright freshly polished copper surface and an opposing face of copper, rendered almost black by oxidation, was such as to require for the neutralising potential in my mode of experimenting * about one-half of the potential of a Daniell's cell.

Some not hitherto published experiments with polished silver plates, which I made fifteen years ago, showed me very startlingly an electric influence from a quite infinitesimal whiff of iodine vapour. The effect on the contact-electricity quality of the surface seems to go on continuously from the first lodgment, to all other tests quite imperceptible, of a few atoms or molecules of the attacking substance (oxygen, or iodine, or sulphur, or chlorine, for example), and to go on increasing until some such thickness as 1-30,000th or 1-40,000th of a centimetre is reached by the film of oxide or iodide, or whatever it may be that is formed.

The subject is one that deserves much more of careful experimental work and measurement than has hitherto been devoted to it. I allude to it at present to point out to you how it is that by this

* First described in a letter to Joule, published in the 'Proceedings of the Literary and Philosophical Society of Manchester' of Jan. 21, 1862, where also I first pointed out the demonstration of a limit to the size of molecules from measurements of contact-electricity. The mode of measurement is more fully described in the article of 'Nature' (vol. xxiii. p. 567), referred to above.

electric action we are enabled as it were to sound the depth of the ocean of molecules attracted to the metallic surface by the vapour or gas entering into combination with it.

When we come to thicknesses of considerably less than a wave-length we find solid metals becoming transparent. Through the kindness of Prof. Dewar I am able to show you some exceedingly thin films of measured thicknesses of platinum, gold, and silver, placed on glass plates. The platinum is of $1 \cdot 9 \times 10^{-5}$ cm. thickness, and is quite opaque; but here is a gold film of about the same thickness, which is transparent to the electric light, as you see, and transmits the beautiful green colour which you see on the screen. The thickness of this gold ($1 \cdot 9$, or nearly 2) is just half the wave-length of violet light in air. This transparent gold, transmitting green light to the screen as you see, at the same time reflects yellow light to the ceiling. Now I will show you the silver. It is thinner, being only $1 \cdot 5 \times 10^{-5}$ of a centimetre thick, or $\frac{3}{8}$ths of the air-wave-length of violet light. It is quite opaque to the electric light so far as our eyes allow us to judge, and reflects all the light up to the ceiling. It is not wonderful that it should be opaque; we might wonder if it were otherwise; but there is an invisible ultra-violet light of a small range of wave-lengths, including a zinc line of air wave-length $3 \cdot 4 \times 10^{-5}$, which this silver film transmits. For that particular light the silver film of $1 \cdot 5 \times 10^{-5}$ thickness is transparent. The image which you now see on the screen is a magic lantern representation of the self-photographed spectrum of light that actually came through that silver. You see the zinc line very clear across it near its middle. Here then we have gold and silver transparent. The silver is opaque for all except that very definite light of wave-lengths from about $3 \cdot 07$ to $3 \cdot 32$.

The different refrangibility of different colours is a result of observation of vital importance in the question of the size of atoms. You now see on the screen before you a prismatic spectrum, a well-known phenomenon produced by the differences of the refractions of the different colours in traversing the prism. The explanation of it in the undulatory theory of light has taxed the powers of mathematicians to the utmost. Look first, however, to what is easy and made clear by that diagram (Fig. 3) before you, and you will easily understand that refraction depends on difference of velocity of propagation of light in the two transparent mediums concerned. The angles in the diagram are approximately correct, for refraction at an interface between air or vacuum and flint glass; and you see that in this case the velocity of propagation is less in the denser medium. The more refractive medium (not always the denser) of the two has the less velocity for light transmitted through it. The "refractive index" of any transparent medium is the ratio of the velocity of propagation in the ether to the velocity of propagation in the transparent substance.

Now that the velocity of the propagation of light should be dif-

ferent in different mediums, and should in most cases be smaller in the denser than in the less dense medium, is quite what we should, according to dynamical principles, expect from any conceivable constitution of the luminiferous ether and of palpable transparent substance. But that the velocity of propagation in any one transparent substance should be different for light of different colours, that is to say, of different periods of vibration, is not what we should expect, and could not possibly be the fact if the medium is homogeneous, without any limit as to the smallness of the parts of which the qualities are compared. The fact that the velocity of propagation *does* depend on the period, gives what I believe to be irrefragable proof that the substance

Fig. 3.

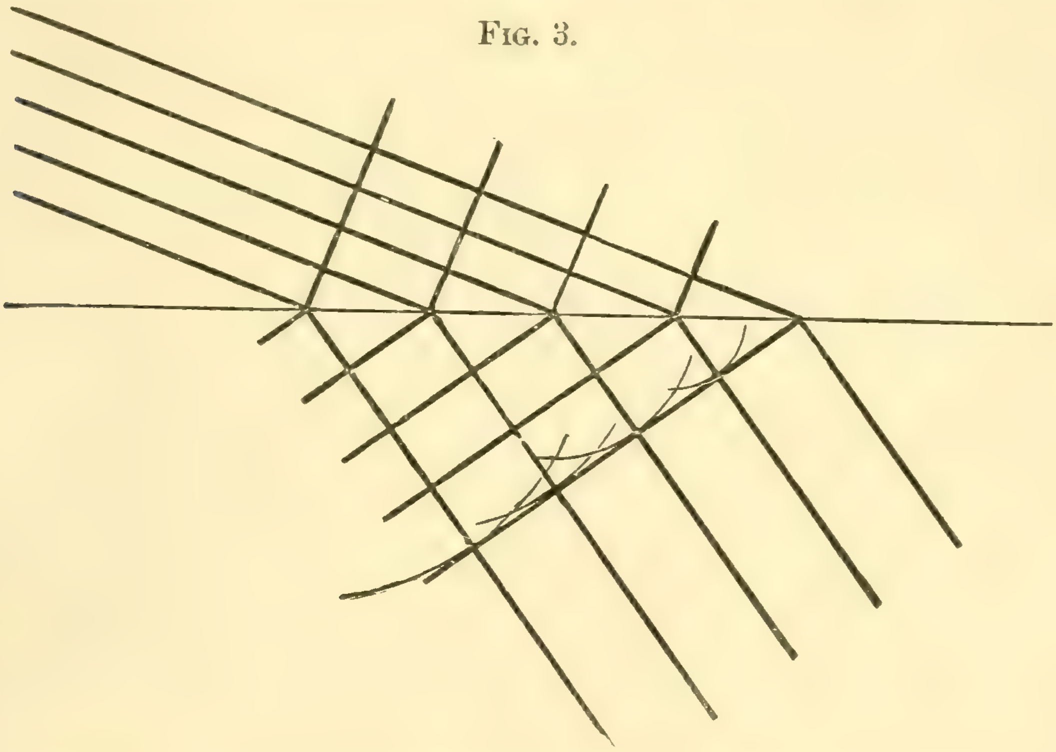


Diagram of Huyghen's construction for wave front of refracted light.
Drawn for light passing from air to flint glass.

of palpable transparent matter, such as water, or glass, or the bisulphuret of carbon of this prism, whose spectrum is before you, is not infinitely homogeneous; but that, on the contrary, if contiguous portions of any such medium, any medium in fact which can give the prismatic colours, be examined at intervals not incomparably small in comparison with the wave-lengths, utterly heterogeneous quality will be discovered; such heterogeneousness as that which we understand, in palpable matter, as the difference between solid and fluid; or between substances differing enormously in density; or such heterogeneousness as differences of velocity and direction of motion, in different positions of a vortex ring in an homogeneous liquid; or such differences of material occupying the space examined, as we find in a great mass of brick building when we pass from brick to brick through mortar (or through *void*, as we too often find in Scotch-built domestic brick chimneys).

Cauchy was, I believe, the first of mathematicians or naturalists to allow himself to be driven to the conclusion that the refractive dispersion of light can only be accounted for by a finite degree of molecular coarse-grainedness in the structure of the transparent refracting matter; and as, however we view the question, and however much we may feel compelled to differ from the details of molecular structure and molecular inter-action assumed by Cauchy, we remain more and more surely fortified in his conclusion, that finite grainedness of transparent palpable matter is the cause of the difference of the velocity of different colours of light propagated through it, we must regard Cauchy as the discoverer of the dynamical theory of the prismatic colours.

But now we come to the grand difficulty of Cauchy's theory.* Look at this little Table (Table II.), and you will see in the heading the formula which gives the velocity, in terms of the number of particles to the wave-length, supposing the medium to consist of equal particles arranged in cubic order, and each particle to attract its six

Table II.—Velocity (V) according to Number (N) of Particles in Wave-Length.

N.	$V\left(=100\dfrac{\sin(\pi/N)}{\pi/N}\right)$.
2	63·64
4	90·03
8	97·45
12	98·86
16	99·36
20	99·59
∞	100·00

nearest neighbours, with a force varying directly as the excess of the distance between them, above a certain constant line (the length of which is to be chosen, according to the degree of compressibility possessed by the elastic solid, which we desire to represent by a crowd of mutually interacting molecules). If you suppose particles of real matter arranged in the cubic order, and six steel wire spiral springs, or elastic indiarubber bands, to be hooked on to each particle and stretched between it and its six nearest neighbours, the postulated force may be produced in a model with all needful accuracy; and if we could but successfully wish the theatre of the Royal Institution conveyed to the centre of the earth and kept there for five minutes, I should have great pleasure in showing you a model of an elastic solid thus constituted, and showing you waves propa-

* For an account of the dynamical theory of the "Dispersion of Light," see 'View of the Undulatory Theory as applied to the Dispersion of Light,' by the Rev. Baden Powell, M.A., &c. (London, 1841.)

gated through it, as are waves of light in the luminiferous ether. Gravity is the inconvenient accident of our actual position which prevents my showing it to you here just now. But instead, you have

Fig. 4.

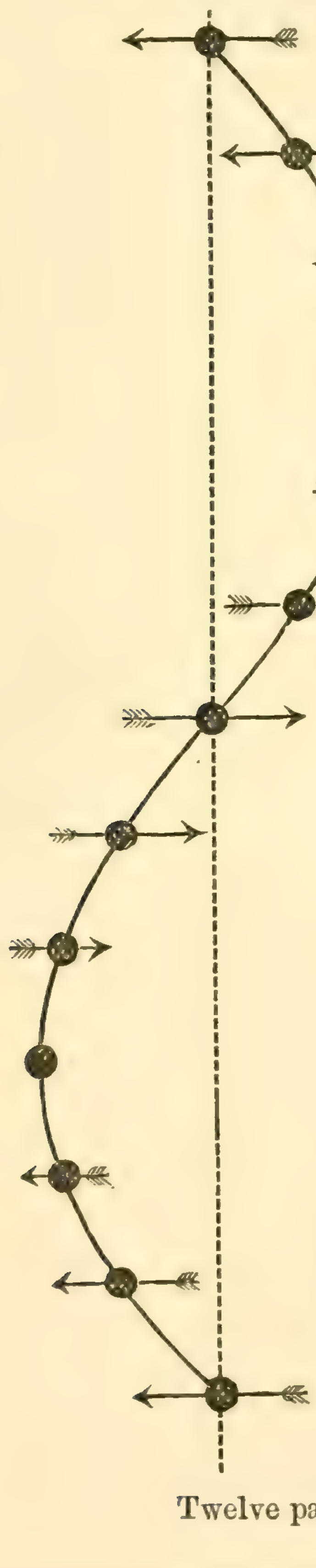
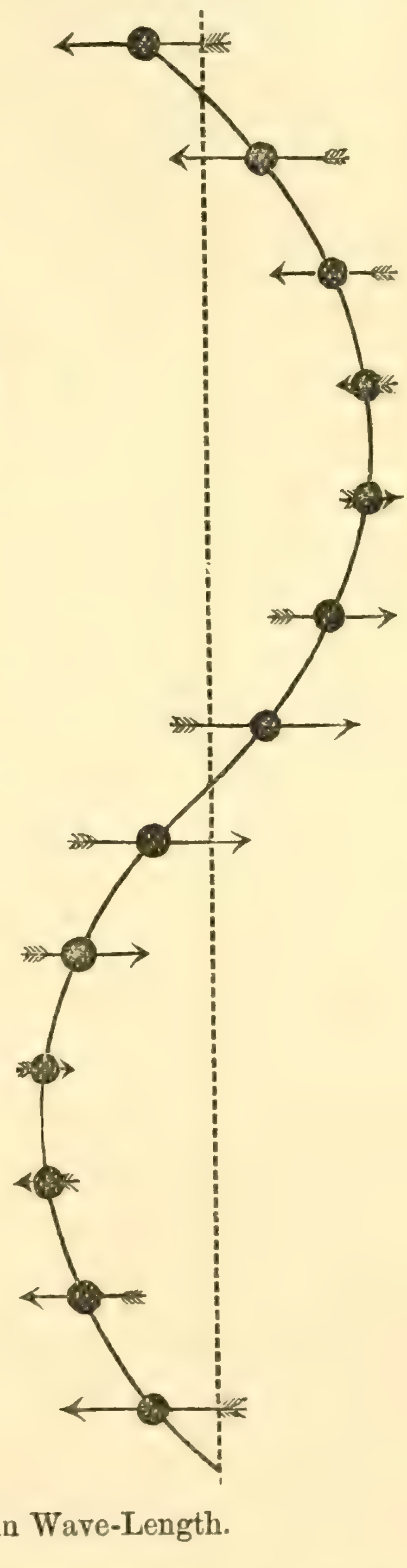


Fig. 5.

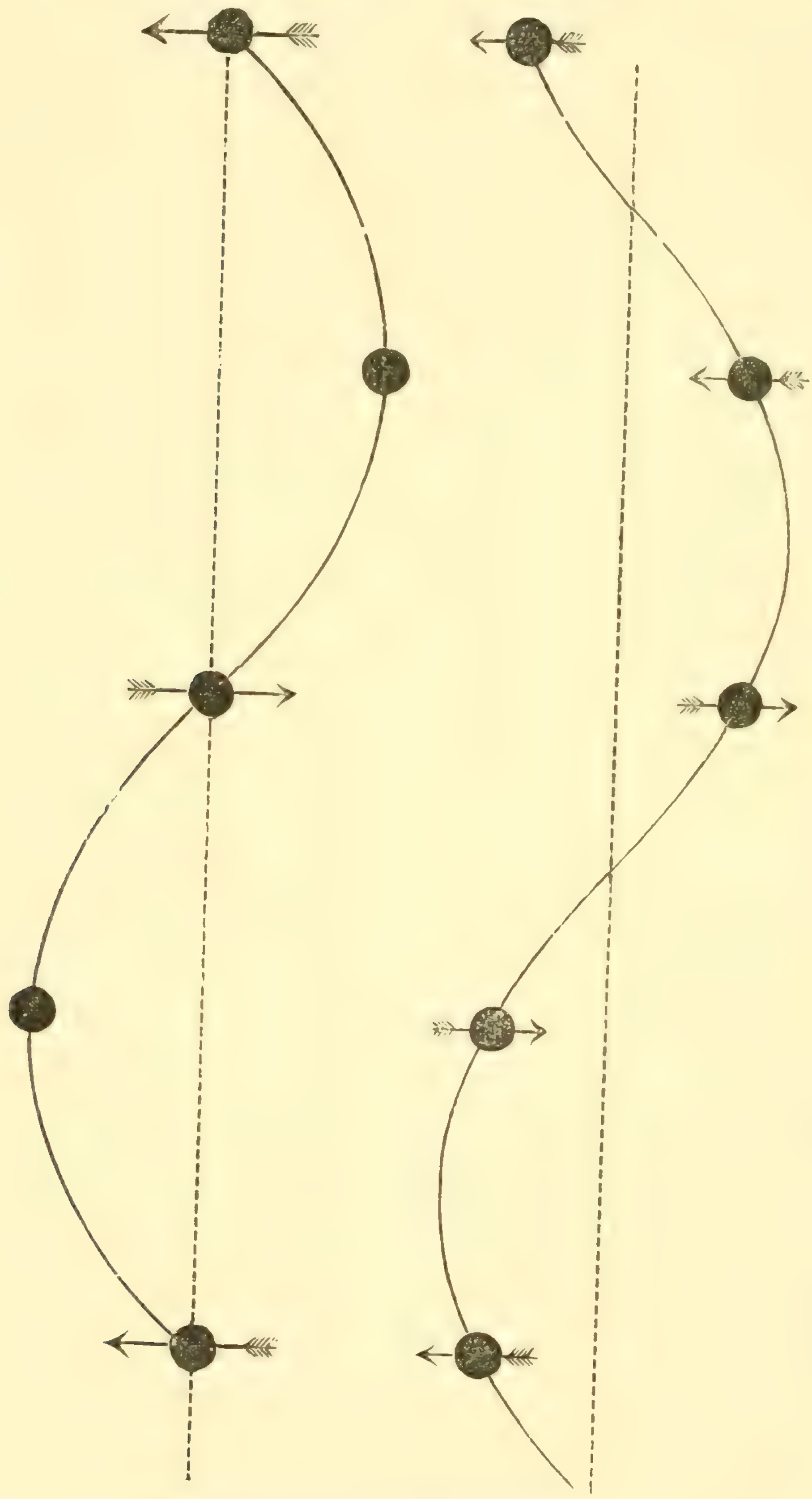


Twelve particles in Wave-Length.

these two wave-models (see Fig. 2), each of which shows you the displacements and motions of a line of particles in the propagation

of a wave through our imaginary three-dimensional solid, the line of molecules chosen being those which in equilibrium are in one direct

Fig. 6. Fig. 7.



Four particles in Wave-Length.

straight line of the cubic arrangement, and the supposed wave having its wave front perpendicular to this line, and the direction of its

vibration the direction of one of the other two direct lines of the cubic arrangement.

You have also before you this series of diagrams (Figs. 4 to 9) of

Fig. 8.

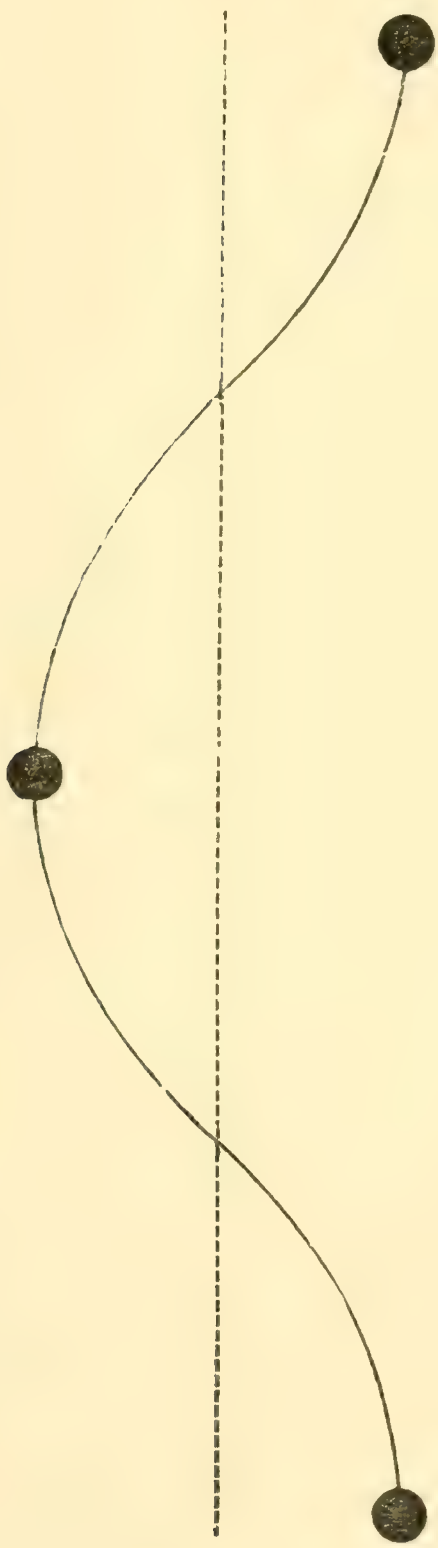
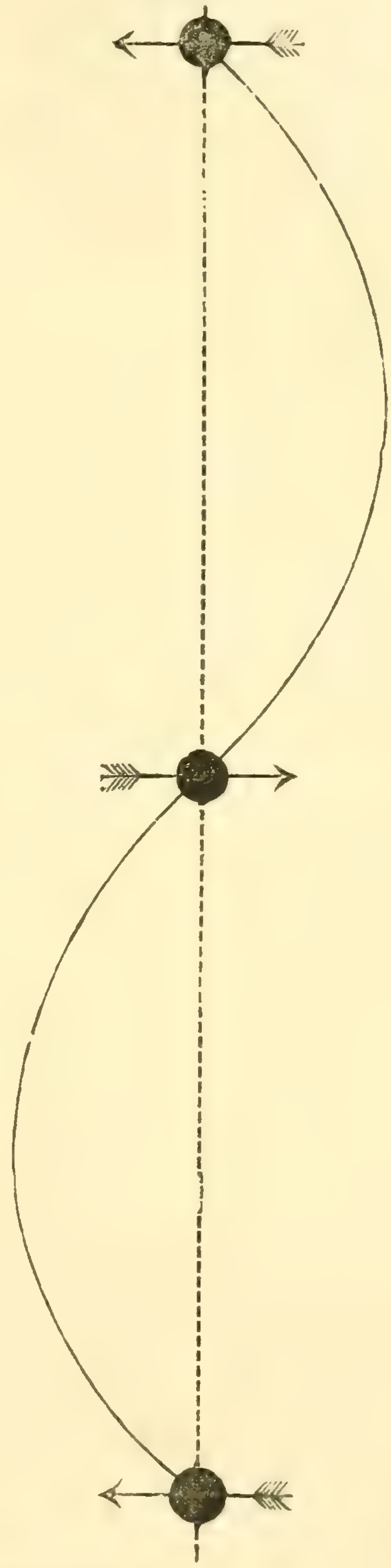


Fig. 9.



Two particles in Wave-Length.

waves in a molecularly-constituted elastic solid. These two diagrams (Figs. 4 and 5) illustrate a wave in which there are twelve molecules in the wave-length; this one (Fig. 4) showing (by the length and

position of the arrows) the magnitude and direction of velocity of each molecule at the instant when one of the molecules is on the crest of the wave, or has reached its maximum displacement; that one (Fig. 5) showing the magnitude and direction of the velocities after the wave has advanced such a distance as (in this case equal to 1-24th of the wave-length) to bring the crest of the wave to midway between two molecules. This pair of diagrams (Figs. 6 and 7) shows the same for waves having four molecules in the wave-length, and this pair (Figs. 8 and 9) for a wave having two molecules in the wave-length.

The more nearly this critical case is approached, that is to say, the shorter the wave-length down to the limit of twice the distance from molecule to molecule, the less becomes the difference between the two configurations of motion constituted by waves travelling in opposite directions. In the extreme or critical case the difference is annulled, and the motion is not a wave motion, but a case of what is often called "standing vibration." Before I conclude this evening I hope to explain in detail the kind of motion which we find instead of wave-motion (become mathematically imaginary), when the vibrational period of the exciter is anything less than the critical value, because this case is of extreme importance and interest in physical optics, according to Stokes's hitherto unpublished explanation of phosphorescence.

This supposition of each molecule acting with direct force only on its nearest neighbour is not exactly the postulate on which Cauchy works. He supposes each molecule to act on all around it, according to some law of rapid decrease as the distance increases; but this must make the influence of coarse-grainedness on the velocity of propagation smaller than it is on the simple assumption realised in the models and diagrams before you, which therefore represents the extreme limit of the efficacy of Cauchy's unmodified theory to explain dispersion.

Now, by looking at the little table (Table II.) of calculated results, you will see that, with as few as 20 molecules in the wave-length, the velocity of propagation is $99\frac{1}{2}$ per cent. of what it would be with an infinite number of molecules; hence the extreme difference of propagational velocity, accountable for by Cauchy's unmodified theory in its idealised extreme of mutual action limited to nearest neighbours, amounts to 1-200th. Now look at this table (Table III.) of refractive indices, and you see that the difference of velocity of red light A, and of violet light H, amounts in carbon disulphide to 1-17th; in dense flint glass to nearly 1-30th; in hard crown glass to 1-73rd; and in water and alcohol to rather more than 1-100th. Hence, none of these substances can have so many as 20 molecules in the wave-length, if dispersion is to be accounted for by Cauchy's unmodified theory, and by looking back to the little table of calculated results (Table II.), you will see that there could not be more than 12 molecules in the wave-length of violet light in water or alcohol;

TABLE III.—TABLE OF REFRACTIVE INDICES.

Line of Spectrum.	Material.				
	Hard Crown Glass.	Extra dense Flint Glass.	Water at 15° C.	Carbon Disulphide at 11° C.	Alcohol at 15° C.
A	1·5118	1·6391	1·3284	1·6142	1·3600
B	1·5136	1·6429	1·3300	1·6207	1·3612
C	1·5146	1·6449	1·3307	1·6240	1·3621
D	1·5171	1 6504	1·3324	1·6333	1·3638
E	1·5203	1·6576	1·3347	1·6465	1·3661
b	1·5210	1·6591	..	..	..
F	1·5231	1·6442	1·3366	1·6584	1·3683
G	1·5283	1·6770	1·3402	1·6836	1·3720
h	1·5310	1·6836	..	..	..
H	1·5328	1·6886	1·3431	1·7090	1·3751

The numbers in the first two columns were determined by Dr. Hopkinson, those in the last three by Messrs. Gladstone and Dale. The index of refraction of air for light near the line E is 1·000294.

say 10 in hard crown glass; 8 in flint glass; and in carbon disulphide actually not more than 4 molecules in the wave-length, if we are to depend upon Cauchy's unmodified theory for the explanation of dispersion. So large coarse-grainedness of ordinary transparent bodies, solid or fluid, is quite untenable. Before I conclude, I intend to show you, from the kinetic theory of gases, a *superior limit* to the size of molecules, according to which, in glass or in water, there is probably something like 600 molecules to the wave-length, and almost certainly *not fewer* than 2, or 3, or 400. But even without any such definite estimate of a superior limit to the size of molecules, there are many reasons against the admission that it is probable or possible there can be only four, or five, or six, to the wave-length. The very drawing, by Nobert, of 4000 lines on a breadth of a millimetre, or at the rate of 40,000 to the centimetre, or about two to the ether wave-length of blue (F) light,* seems quite to negative the idea of any such possibility of only five or six molecules to the wave-length, even if we were not to declare against it from theory and observation of the reflection of light from polished surfaces.

We must then find another explanation of dispersion. I believe there is another explanation. I believe that, while giving up Cauchy's unmodified theory of dispersion, we shall find that the same general principle is applicable, and that by imagining each molecule to be loaded in a certain definite way by elastic connection with heavier matter—each molecule of the ether to have, in palpable transparent matter, a small fringe so to speak of particles, larger and larger in

* Loschmidt, "quoting from the Zollvereins department of the London International Exhibition of 1862, p. 83, and from Harting 'On the Microscope,' p. 881," 'Sitzungsberichte der Wiener Akademie Math. Phys.,' 1865. Vol. iii.

their successive order, elastically connected with it—we shall have a rude mechanical explanation, realisable by the notably easy addition of the proper appliances to the dynamical models before you, to account for refractive dispersion in an infinitely fine-grained structure. It is not seventeen hours since I saw the possibility of this explanation. I think I now see it perfectly, but you will excuse my not going into the theory more fully under the circumstances.* The difficulty of Cauchy's theory has weighed heavily upon me when thinking of bringing this subject before you. I could not bring it before you and say there are only four particles in the wave-length, and I could not bring it before you without saying there is some other explanation. I believe another explanation is distinctly to be had in the manner I have slightly indicated.

Now look at those beautiful distributions of colour on the screen before you. They are diffraction spectrums from a piece of glass ruled with 2000 lines to the inch. And again look, and you see one diffraction spectrum by reflection from one of Rutherford's gratings, in which there are 17,000 lines to the inch on polished speculum-metal. The explanation by "interference" is substantially the same as that which the undulatory theory gives for Newton's rings of light reflected from the two surfaces, which you have already seen. Where light-waves from the apertures between the successive bars of the grating reach the screen in the same phase, they produce light; there, again, where they are in opposite phases, they produce darkness.

The beautiful colours which are produced depend on the places of conspiring and opposing vibrations on the screen, being different for light-waves of different wave-lengths; and it is by the measurements of the dimensions of a diffraction spectrum such as the first set you saw (or of finer spectrums from coarser gratings) that Fraunhofer first determined the wave-lengths of the different colours.

I have now, closely bearing on the question of the size of atoms, thanks to Dr. Tyndall, a most beautiful and interesting experiment to show you—the artificial "blue sky," produced by a very wonderful effect of light upon matter, which he discovered. We have here an empty glass tube—it is "optically void." A beam of electric light passes through it now, and you see nothing. Now the light is stopped, and we admit vapour of carbon disulphide into the tube. There is now introduced some of this vapour to about 3 inches pressure, and there is also introduced, to the amount of 15 inches pressure, air impregnated with a little nitric acid, making in all rather less than the atmospheric pressure. What is to be illustrated here is the presence of molecules of substances produced by the decomposition of carbon disulphide by the light. At present you see nothing in the tube; it still continues to be, as before the admission of the vapours,

* Farther examination has seemed to me to confirm this first impression; and in a paper on the Dynamical Theory of Dispersion, read before the Royal Society of Edinburgh, on the 5th of March, I have given a mathematical investigation of the subject.—W. T., March 16, 1883.

optically transparent; but gradually you will see an exquisite blue cloud. That is Tyndall's "blue sky." You see it now. I take a Nicol's prism, and by looking through it I find the azure light coming from the vapours in any direction perpendicular to the exciting beam of light to be very completely polarised in the plane through my eye and the exciting beam. It consists of light-vibrations in one definite direction, and that, as finally demonstrated by Professor Stokes, it seems to me beyond all doubt, through reasoning on this phenomenon of polarisation,* which he had observed in various experimental arrangements giving minute solid or liquid particles scattered through a transparent medium, must be the direction perpendicular to the plane of polarisation.

What you are now about to see, and what I tell you I have seen through the Nicol's prism, is due to what I may call secondary or derived waves of light diverging from very minute liquid spherules, condensed in consequence of the chemical decomposing influence exerted by the beam of light on the matter in the tube, which was all gaseous when the light was first admitted.

To understand these derived waves, first you must regard them as due to motion of the ether round each spherule; the spherule being almost absolutely fixed, because its density is enormously greater than

---

* Extract from Professor Stokes's paper "On the Change of Refrangibility of Light," read before the Royal Society, May 27th, 1852, and published in the 'Transactions' for that date:—

"§ 183. Now this result appears to me to have no remote bearing on the question of the directions of the vibration in polarised light. So long as the suspended particles are large compared with the waves of light, reflection takes place as it would from a portion of the surface of a large solid immersed in the fluid, and no conclusion can be drawn either way. But if the diameters of the particles be small compared with the length of a wave of light, it seems plain that the vibrations in a reflected ray cannot be perpendicular to the vibrations in the incident ray. Let us suppose for the present, that in the case of the beams actually observed, the suspended particles were small compared with the length of a wave of light. Observation showed that the reflected ray was polarised. Now all the appearances presented by a plane polarised ray are symmetrical with respect to the plane of polarisation. Hence we have two directions to choose between for the direction of the vibrations in the reflected ray, namely, that of the incident ray, and a direction perpendicular to both the incident and the reflected rays. The former would be necessarily perpendicular to the directions of vibration in the incident ray, and therefore we are obliged to choose the latter, and consequently to suppose that the vibrations of plane polarised light are perpendicular to the plane of polarisation, since experiment shows that the plane of polarisation of the reflected ray is the plane of reflection. According to this theory, if we resolve the vibrations in the [horizontal] incident ray horizontally and vertically, the resolved parts will correspond to the two rays, polarised respectively in and perpendicularly to the plane of reflection, into which the incident ray may be conceived to be divided, and of these the former alone is capable of furnishing a . . . . . ray reflected vertically upwards [to be seen by an eye above the line of the incident ray, and looking vertically downwards]. And, in fact, observation shows that, in order to quench the dispersed beam, it is sufficient, instead of analysing the reflected light, to polarise the incident light in a plane perpendicular to the plane of reflection."

that of the ether surrounding it. The motion that the ether had in virtue of the exciting beam of light alone, before the spherules came into existence, may be regarded as being compounded with the motion of the ether relatively to each spherule, to produce the whole resultant motion experienced by the ether when the beam of light passes along the tube, and azure light is seen proceeding from it laterally. Now this second component motion is clearly the same as the whole motion of the ether would be, if the exciting light were annulled and each spherule kept vibrating in the opposite direction, to and fro through the same range as that which the ether in its place had, in virtue of the exciting light, when the spherule was not there.

Supposing now, for a moment, that without any exciting beam at all, a large number of minute spherules are all kept vibrating through very small ranges * parallel to one line. If you place your eye in the plane through the length of the tube and perpendicular to that line, you will see light from all parts of the tube, and this light which you see will consist of vibrations parallel to that line. But if you place you eye *in* the line of the vibration of a spherule, situated about the middle of the tube, you will see no light in that direction; but keeping your eye in the same position, if you look obliquely towards either end of the tube, you will see light fading into darkness, as you

---

* In the following question of the recent Smith's Prize Examination at Cambridge (paper of Tuesday, Jan. 30, 1883), the dynamics of the subject, and particularly the motion of the ether produced by keeping a single spherule embedded in it vibrating to and fro in a straight line, are illustrated in parts (a) and (d):—

"8. (a) From the known phenomenon that the light of a cloudless blue sky, viewed in any direction perpendicular to the sun's direction, is almost wholly polarised in the plane through the sun, assuming that this light is due to particles of matter of diameters small in comparison with the wave-length of light, prove that the direction of the vibrations of plane polarised light is perpendicular to the plane of polarisation.

"(b) Show that the equations of motion of a homogeneous isotropic elastic solid of unit density, are

$$\frac{d^2\alpha}{dt^2} = (k + \tfrac{1}{3}n)\frac{d\delta}{dx} + n\nabla^2\alpha,$$

$$\frac{d^2\beta}{dt^2} = (k + \tfrac{1}{3}n)\frac{d\delta}{dy} + n\nabla^2\beta,$$

$$\frac{d^2\gamma}{dt^2} = (k + \tfrac{1}{3}n)\frac{d\delta}{dz} + n\nabla^2\gamma,$$

where k denotes the modulus of resistance to compression; n the rigidity-modulus; α, β, γ, the components of displacement at (x, y, z, t); and

$$\delta = \frac{d\alpha}{dx} + \frac{d\beta}{dy} + \frac{d\gamma}{dz},$$

$$\nabla^2 = \frac{a^2}{dx^2} + \frac{d^2}{dy^2} + \frac{d^2}{dz^2}.$$

"(c) Show that every possible solution is included in the following:—

$$\alpha = \frac{d\phi}{dx} + u, \quad \beta = \frac{d\phi}{dy} + v, \quad \gamma = \frac{d\phi}{dz} + w,$$

turn your eye from either end towards the middle. Hence, if the exciting beam be of plane polarised light—that is to say, light of which all the vibrations are parallel to one line—and if you look at the tube in the direction perpendicular to this line and to the length of the tube, you will see light of which the vibrations will be parallel to that same line. But if you look at the tube in any direction parallel to this line, you will see no light; and the line along which you see no light is the direction of the vibrations in the exciting beam; and this direction, as we now see, is the direction perpendicular to what is technically called the plane of polarisation of the light. Here, then, you have Stokes's *experimentum crucis* by which he has answered, as seems to me beyond all doubt, the old vexed question—Whether is the vibration *perpendicular to*, or *in* the plane of polarisation? To show you this experiment, instead of using unpolarised light for the exciting beam, as in the previous experiment, and holding a small Nicol's prism in my hand and telling you what I saw when I looked through it, I place, as is now done, this great Nicol's prism in the course of the beam of light before it enters the tube. I now turn the Nicol's prism into different directions and turn the apparatus round, so that, sitting in all parts of the theatre, you may all see the tube in the proper direction for the successive phenomena of "light," and "no light." You see them now exactly fulfilling the description which I gave you in anticipation. If each of you had a Nicol's prism in your hand, you would learn that when you see light at all, its plane of polarisation is in the plane through your eye and the axis of the tube; and I hope you all now perfectly understand the proof that the direction of vibration is perpendicular to this plane.

Now I want to bring before you something which was taught me

---

where u, v, w are such that

$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0.$$

"Find differential equations for the determination of ϕ, u, v, w. Find the respective wave-velocities for the ϕ-solution, and for the (u, v, w)-solution.

"(*d*) Prove the following to be solutions, and interpret each for values of $r\,[\sqrt{(x^2 + y^2 + z^2)}]$ very great in comparison with λ (the wave-length).

$$(1)\quad \begin{cases} \alpha = \dfrac{d\phi}{dx}, \quad \beta = \dfrac{d\phi}{dy}, \quad \gamma = \dfrac{d\phi}{dz} \\[2ex] \text{where } \phi = \dfrac{1}{r}\sin\dfrac{2\pi}{\lambda}\left[r - t\sqrt{(k + \tfrac{4}{3}n)}\right]. \end{cases}$$

$$(2)\quad \begin{cases} \alpha = 0, \quad \beta = -\dfrac{d\psi}{dz}, \quad \gamma = \dfrac{d\psi}{dy} \\[2ex] \text{where } \psi = \dfrac{1}{r}\sin\dfrac{2\pi}{\lambda}\left[r - t\sqrt{n}\right]. \end{cases}$$

$$(3)\quad \alpha = \left(\frac{2\pi}{\lambda}\right)\psi + \frac{d^2\psi}{dx^2}, \quad \beta = \frac{d^2\psi}{dx\,dy}, \quad \gamma = \frac{d^2\psi}{dx\,dz}.$$

a long time ago by Professor Stokes; and year after year I have begged him to publish it, but he has not done so, and so I have asked

Fig. 10.

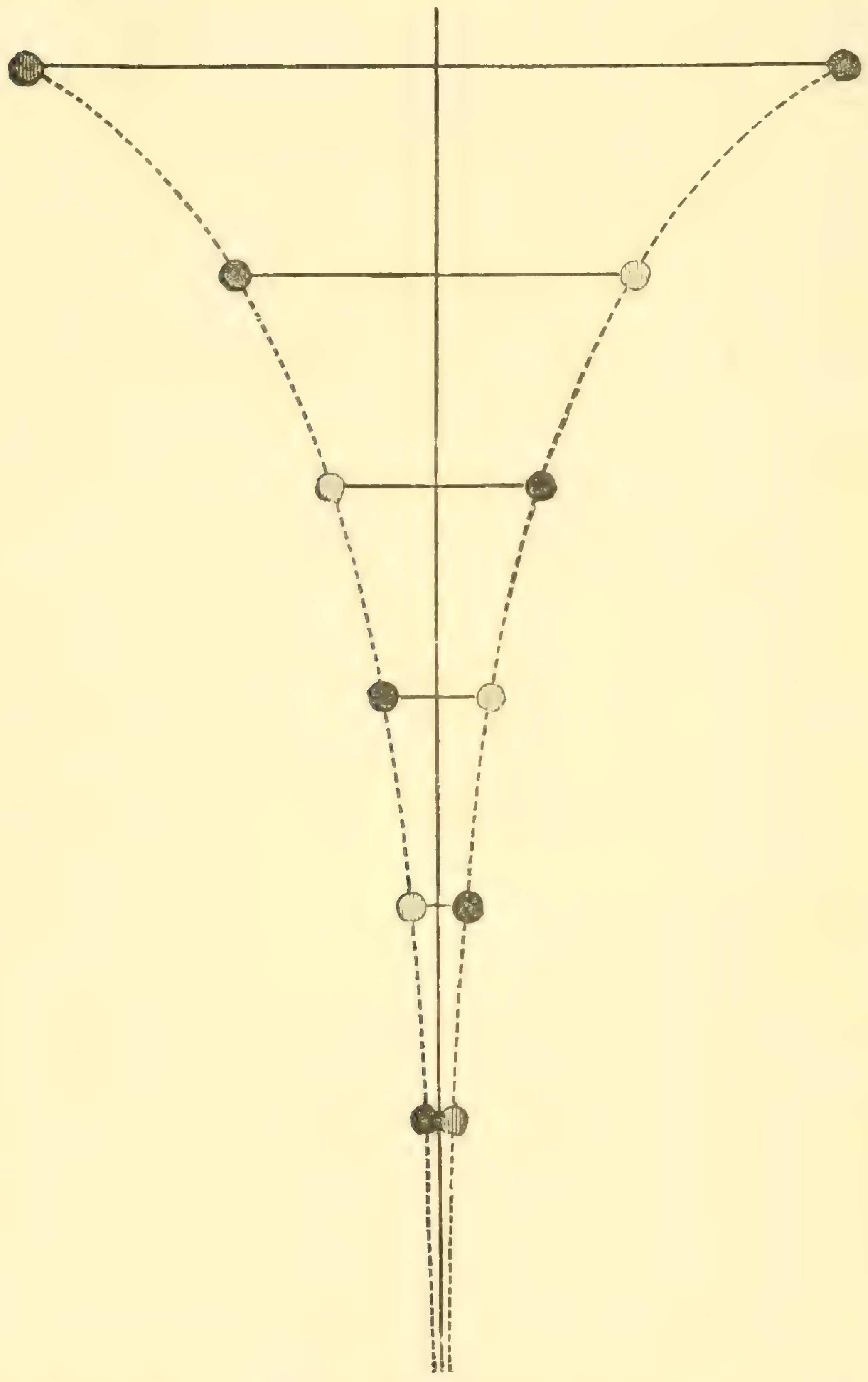


Diagram showing the different amplitudes of vibration of a row of particles oscillating in a period less than their least wave-period.

him to allow me to speak of it to-night. It is a dynamical explanation of that wonderful phenomenon called fluorescence or phosphorescence.

The principle is mechanically represented by this model (described above with reference to Fig. 2). A simple harmonic motion is, as you now see, sustained by my hand in the uppermost bar, in a period of about four seconds. You see that a regular wave-motion travels down the line of molecules represented by these circular disks on the ends of the bars, and the energy continually given to the top bar, by my hand, is continually consumed in heating the basin of treacle and water at the foot. I now remove my hand and leave the whole system to itself. The very considerable sum of kinetic and potential energies of the large masses and spiral springs, attached to the top bar, is gradually spent in sending the diminishing series of waves down the line, and is ultimately converted into heat in the treacle and water. You see that about half of the amplitude of vibration, and therefore three-fourths of the energy, is lost in half a minute.

You will see on quickening the oscillation how very different the result will be. The quick oscillations which I now give to the top bar (the period having been reduced to about one and a half seconds), is incapable of sending waves along the line of molecules; and it is that rapid oscillation of the particles which, according to Stokes, constitutes latent or stored-up light. Remark now that when I remove my hand from the top bar, as no waves travel down the line, no energy is spent in the treacle; and the vibration goes on for ever (or, to be more exact, say for one minute) as you see, with *no loss* (or, to be quite in accordance with what we see, let me say scarcely any sensible loss). This is a mechanical model correctly illustrating the dynamical principle of Stokes's explanation of phosphorescence or stored-up light, stored as in the now well-known luminous paint, of which you see the action in this specimen, and in the phosphorescent sulphides of lime in these glass tubes kindly lent by Mr. De La Rue. (Experiment shown.)

Now I will show you Stokes's phenomenon of *fluorescence* in a piece of uranium glass. I hold it in the beam from the electric lamp dispersed by the prism as you see. You see the uranium glass now visible by being illuminated by invisible rays. The rays by which it is illuminated even before it comes into the visible rays are manifestly invisible so far as the screen receiving the spectrum is a test of visibility; because the uranium glass, and my hands holding it, throw no shadow on the screen. Also you see the uranium glass which I hold in my hand in the ultra-violet light, while you do not see my hand. I now bring it nearer the place where you see the air (or rather the dust in it) illuminated by the violet light: still no shadow on the screen, but the uranium glass in my hand glowing more brilliantly with its green light of very mixed constitution, consisting of waves of longer periods than that of the ultra-violet, which the incident light, of shorter period than that of violet light, causes the particles of the uranium glass to emit. This light is altogether unpolarised. It was the absolute want of polarisation, and the fact of

its periods being all less than those of the exciting light, that led Stokes to distinguish this illumination, which you see in the uranium glass,* from the mere molecular illumination (always polarised partially if not completely, and always of the same period as that of the exciting light) which we were looking at previously in Dr. Tyndall's experiment.

Stokes gave the name of fluorescence to the glowing with light of larger period than the exciting light, because it is observed in fluor spar, and he wished to avoid all hypothesis in his choice of a name. He pointed out a strong resemblance between it and the old known phenomenon of phosphorescence; but he found some seeming contrasts between the two, which prevented him from concluding fluorescence to be in reality a case of phosphorescence.

In the course of a comparison between the two phenomena (sections 221 to 225 of his 1852 paper), the following statement is given:—"But by far the most striking point of contrast between the two phenomena consists in the apparently instantaneous commencement and cessation of the illumination, in the case of internal dispersion when the active light is admitted and cut off. There is nothing to create the least suspicion of any appreciable duration in the effect. When internal dispersion is exhibited by means of an electric spark, it appears no less momentary than the illumination of a landscape by a flash of lightning. I have not attempted to determine whether any appreciable duration could be made out by means of a revolving mirror." The investigation here suggested has been actually made by Edmund Becquerel, and the question—Is there any appreciable duration in the glow of fluorescence?—has been answered affirmatively by this beautiful and simple little machine before you, which he invented for the purpose. The experiment giving the answer is most interesting, and I am sure you will see it with pleasure. It consists of a flat circular box, with two holes facing one another in the flat sides near the circumference; inside are two disks, carried by a rapidly revolving shaft, by which the holes are alter-

* The same phenomenon is to be seen splendidly in sulphate of quinine. An interesting experiment may be made by writing on a white paper screen, with a finger or a brush dipped in a solution of sulphate of quinine. The marking is quite imperceptible in ordinary light; but if a prismatic spectrum be thrown on the screen, with the ultra-violet invisible light on the part which had been written on with the sulphate of quinine, the writing is seen glowing brilliantly with a bluish light, and darkness all round. The phenomenon presented by sulphate of quinine and many other vegetable solutions, and some minerals, as, for instance, fluor spar, and various ornamental glasses, as a yellow Bohemian glass, called in commerce "canary glass" (giving a dispersed greenish light), had been discovered by Sir David Brewster ('Transactions,' Royal Society of Edinburgh, 1833, and British Association, Newcastle, 1838), and had been investigated also by Sir John Herschel, and by him called "epipolic dispersion" ('Phil. Trans.,' 1845). A complete experimental analysis of the phenomenon, showing precisely what it was that the previous observers had seen, and explaining many singularly mysterious things which they had noticed, was made by Stokes, and described in his paper, "On the Change of Refrangibility of Light" ('Phil. Trans.,' May 27, 1852).

nately shut and opened; one open when the other is closed, and *vice versâ*. A little piece of uranium glass is fixed inside the box between the two holes, and a beam of light from the electric lamp falls upon one of the holes. You look at the other.

Now when I turn the shaft slowly you see nothing. At this instant the light falls on the uranium glass through the open hole far from you, but you see nothing, because the hole next you is shut. Now the hole next you is open, but you see nothing, because the hole next the light is shut, and the uranium glass shows no perceptible after-glow as arising from its previous illumination. This agrees exactly with what you saw when I held the large slab of uranium glass in the ultra-violet light of the prismatic spectrum. As long as I held the uranium glass there you saw it glowing; the moment I took it out of the invisible light it ceased to glow. The "moment" of which we were then cognisant may have been the tenth of a second. If the uranium glass had continued to glow sensibly for the twentieth or the fiftieth of a second, it would have seemed to our slow-going sense of vision to cease the moment it was taken out. Now I turn the wheel at such a rate that the hole next you is open about a fiftieth of a second after the uranium glass was bathed in light; still you see nothing. I turn it faster and faster, and it now begins to glow, when the hole next you is open about the two-hundredth of a second after the immediately preceding admission of light by the other hole. I turn it faster and faster, and it glows more and more brightly, till now it is glowing like a red coal; further augmentation of the speed shows, as you see, but little difference in the glow.

Thus it seems that fluorescence is essentially the same as phosphorescence; and we may expect that substances will be found continuously bridging over the difference of quality between this uranium glass, which glows only for a few thousandths of a second, and the luminous sulphides which glow for hours or days or weeks after the cessation of the exciting light.

The most decisive and discriminating method of estimating the size of atoms I have left until my allotted hour is gone—that founded on the kinetic theory of gases. Here is a diagram (Fig. 11) of a crowd of atoms or molecules showing, on a scale of 1,000,000 to 1, all the molecules of air, of which the centres may at any instant be in a space of a square of 1-10,000th of a centimetre side and 1-100,000,000th of a centimetre thick. The side of the square you see in the diagram is a metre, and represents 1-10,000th of a centimetre. The diagram shows just 100 molecules, being 1-10,000th of the whole number of particles (10^6) in the cube of 1-10,000th centimetre, or all the molecules in a slice of 1-10,000th of the thickness of that cube. Think of a cube filled with particles, like these glass balls,* scattered at random

* The piece of apparatus now exhibited, illustrated the collisions taking place between the molecules of gaseous matter and the diffusion of one gas into another. It consisted of a board of about one metre square, perforated with

through a space equal to 1000 times the sum of their volumes. Such a crowd may be condensed (just as air may be condensed) to 1-1000th of its volume, but this condensation brings the molecules into contact. Something comparable with this may be imagined to be the condition of common air of ordinary density, as in our atmosphere. The diagram with size of each molecule, which, if shown in it to scale, would be 1 millimetre (or too small to be seen by you), to represent

Fig. 11.

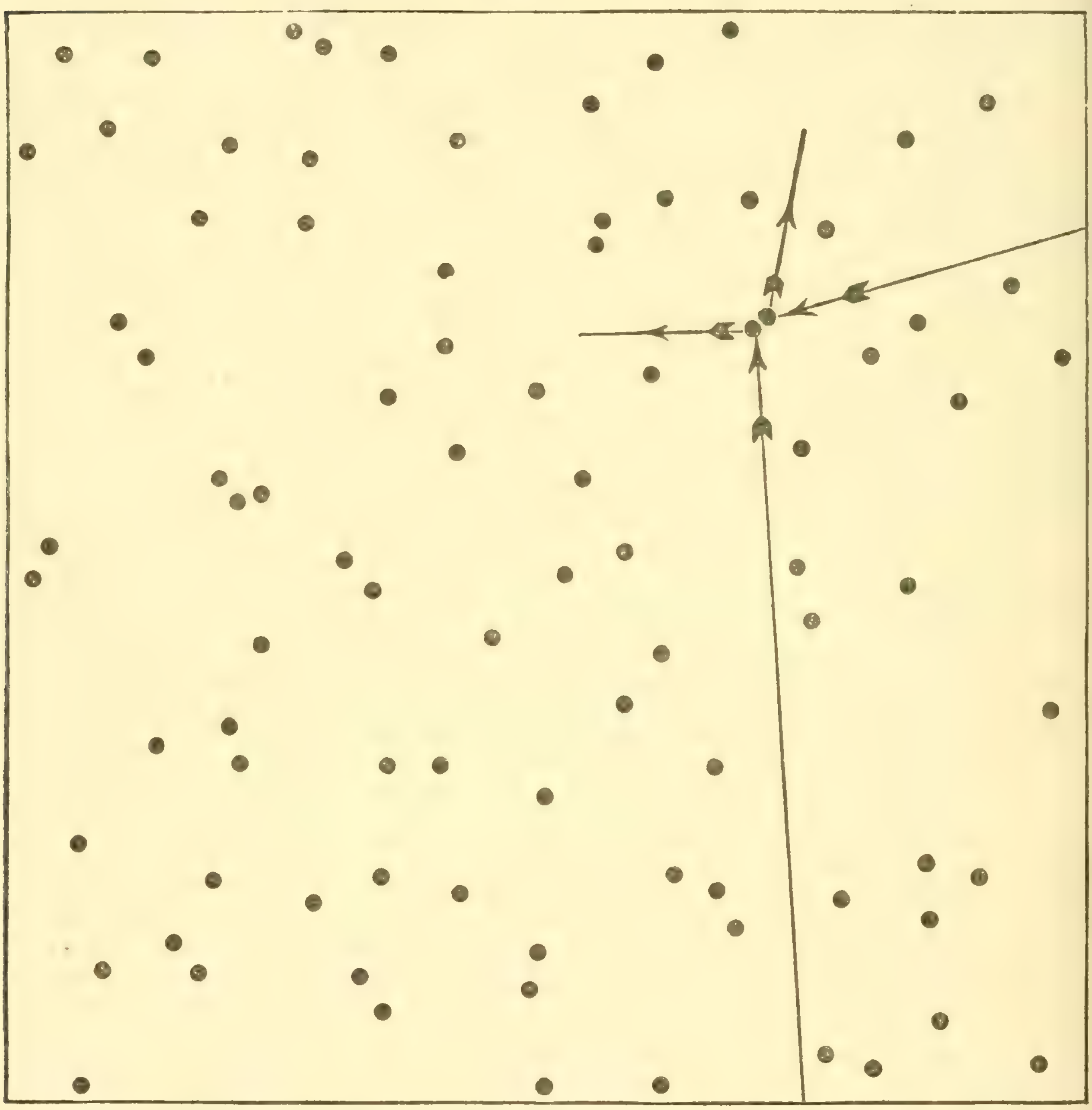


Diagram illustrating the number of molecules in a space of 1-10,000th of a centimetre square and 1-100,000,000th of a centimetre thick.

---

100 holes in ten rows of ten holes each. From each hole was suspended a cord five metres long. To the lower end of each cord, in five contiguous rows, there was secured a blue coloured glass ball of four centimetres diameter; and similarly to each cord of the other five rows, a red coloured ball of the same size. A ball from one of the outer rows was pulled aside, and, being set free, it plunged in amongst the others, causing collisions throughout the whole plane in which the suspended balls were situated.

an actual diameter 1-10,000,000th of a centimetre, represents a gas in which a condensation of 1 to 10 linear, or 1 to 1000 in bulk, would bring the molecules close together.

Now you are to imagine the particles moving in all directions, each in a straight line until it collides with another. The average length of free path is 10 centimetres in our diagram, representing 1-100,000th of a centimetre in reality. And to suit the case of atmospheric air of ordinary density and at ordinary pressure you must suppose the actual velocity of each particle to be 50,000 centimetres per second, which will make the average time from collision to collision 1-5,000,000,000th of a second.

The time is so far advanced that I cannot speak of the details of this exquisite kinetic theory, but I will just say that three points investigated by Maxwell and Clausius, viz. the viscosity or want of perfect fluidity of gases, the diffusion of gases into one another, and the diffusion of heat through gases—all these put together give an estimate for the average length of the free path of a molecule. Then a beautiful theory of Clausius enables us, from the average length of the free path, to calculate the magnitude of the atom. That is what Loschmidt has done,* and I, unconsciously following in his wake, have come to the same conclusion; that is, we have arrived at the absolute certainty that the dimensions of a molecule of air are something like that which I have stated.

The four lines of argument which I have now indicated lead all to substantially the same estimate of the dimensions of molecular structure. Jointly they establish, with what we cannot but regard as a very high degree of probability, the conclusion that, in any ordinary liquid, transparent solid, or seemingly opaque solid, the mean distance between the centres of contiguous molecules is less than the 1-5,000,000th, and greater than the 1-1,000,000,000th of a centimetre.

To form some conception of the degree of coarse-grainedness indicated by this conclusion, imagine a globe of water or glass, as large as a football,† to be magnified up to the size of the earth, each constituent molecule being magnified in the same proportion. The magnified structure would be more coarse-grained than a heap of small shot, but probably less coarse-grained than a heap of footballs.

[W. T.]

---

* *Sitzungsberichte* of the Vienna Academy, Oct. 12, 1865, p. 395.

† Or say a globe of 16 centimetres diameter.

GENERAL MONTHLY MEETING,

Monday, February 5, 1883.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

The Duke of Bedford, K.G.
Lord Lawrence.
Major Gerald Edmund Boyle.
Mrs. Henry Bonham Carter.
Joshua Fielden, Esq.
Major Alexander Thomas Fraser, R.E.
Mrs. Clara E. Murchison.

were elected Members of the Royal Institution.

The special thanks of the Members were returned for the following donations for the purchase of a new Gas Engine:—

Warren De La Rue, Esq.	£100
Colonel James Augustus Grant	50
Professor Tyndall	50

The special thanks of the Members were given to Mr. James Wimshurst, for his present of an Electrical Influence Machine constructed by him.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

The Governor-General of India—Palæontologia Indica: Series XIV. Vol. I. Part 3, Fasc. 2. fol. 1881–2.
Geological Survey of India: Records. Vol. XV. Part 4. 8vo. 1882.
The New Zealand Government—Results of a Census, 3 April, 1881. fol. 1882.
The French Government—Documents Inédits sur l'Histoire de France:
Comptes des Bâtiments du Roi sous le règne de Louis XIV. Par J. Guiffrey. Tome I. 4to. 1881.
Lettres de Catherine de Médicis. Par C. Cte. Hector de la Ferrière. Tome I. 4to. 1880.
Melanges Historiques. Tome III. 4to. 1880.
Mémoires des Intendants sur l'état des Généralités, pour l'instruction du Duc de Bourgoyne. Par A. M. de Boislisle. Tome I. 4to. 1881.
Receuil des Chartes de l'Abbaye de Cluny. Par A. Bernard et A. Bruel. Tome II. 4to. 1880.
Accademia dei Lincei, Reale, Roma—Atti, Serie Terza. Vol. VII. Fasc. 1, 2. 4to. 1882.

American Academy of Arts and Sciences—Memoirs, New Series, Vol. X. Part 2. 4to. 1882.
Proceedings, Vol. XVII. 8vo. 1882.
American Philosophical Society—Proceedings, Nos. 110, 111. 8vo. 1881–2.
Arbuthnot, F. Forster, Esq. M.R.I.—Early Ideas: Hindoo Stories. Collected by Anaryan. 8vo. 1881.
Archæological Survey of Southern India—The Amaravati Stupa. By J. Burgess. 4to. Madras, 1882.
Asiatic Society of Bengal—Proceedings, Nos. 7, 8. 8vo. 1882.
Journal, Vol. LI. Part 1, Nos. 3, 4; Part 2, Nos. 2, 3. 8vo. 1882.
Asiatic Society, Royal—Journal, Vol. XV. Part 1. 8vo. 1882.
Astronomical Society, Royal—Monthly Notices, Vol. XLIII. Nos. 1, 2. 8vo. 1882.
Bankers, Institute of—Journal, Vol. III. Parts 5–9; Vol. IV. Part 1. 8vo. 1882–3.
Barr, James, M.D. (the Author)—Reduplication of the Cardiac Sounds. 8vo. 1882.
Boston Society of Natural History—Memoirs, Vol. III. Nos. 4, 5. 4to. 1881–2.
Proceedings, Vol. XX. Part 4; Vol. XXI. Parts 1–3. 8vo. 1881–2.
British Architects, Royal Institute of—Proceedings, 1882–3, Nos. 4–7. 4to.
Brodie, Alexander, Esq. M.R.I. (the Editor)—Reminiscences of Solomon Alex. Hart, R.A. 8vo. 1882. (Privately Printed.)
Chemical Society—Journal for Dec. 1882, Jan. 1883. 8vo.
Crisp, Frederick Arthur, Esq. M.R.I. (the Compiler)—Some Account of the Parish of Stutton. 4to. 1881. (Privately Printed.)
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Series II. Vol. II. Part 6. 8vo. 1882.
Dax: Société de Borda—Bulletins, 2e Serie, Septième Année: Trimestre 4. 8vo. 1882.
Editors—American Journal of Science for Dec. 1882, Jan. 1883. 8vo.
Analyst for Dec. 1882, Jan. 1883. 8vo.
Athenæum for Dec. 1882, Jan. 1883. 4to.
Chemical News for Dec. 1882, Jan. 1883. 4to.
Engineer for Dec. 1882, Jan. 1883. fol.
Horological Journal for Dec. 1882, Jan. 1883. 8vo.
Iron for Dec. 1882, Jan. 1883. 4to.
Nature for Dec. 1882, Jan. 1883. 4to.
Revue Scientifique and Revue Politique et Littéraire for Dec. 1882, Jan. 1883. 4to.
Telegraphic Journal for Dec. 1882, Jan. 1883. fol.
Franklin Institute—Journal, Nos. 684, 685. 8vo. 1882.
Geographical Society, Royal—Proceedings, New Series, Vol. IV. No. 12, Vol. V. No. 1. 8vo. 1882.
Geological Society—Abstracts of Proceedings, 1882–3, Nos. 426–430. 8vo.
Guy, Dr. W. A. F.R.S. (the Author)—The Claims of Science to Public Recognition and Support. 4to. 1882.
Johns Hopkins University—American Journal of Philology, Nos. 1 to 11. 8vo. 1880–2.
American Chemical Journal, Vols. II. III. and IV. Nos. 1 to 4. 8vo. 1880–2.
Annual Report, 1881 and 1882. 8vo.
University Circulars, Nos. 17, 19. 4to. 1882.
Kerr, Mrs. Alexander, M.R.I.—Notes on a Novel Form of Sun Dial. By E. C. Caldwell. 8vo. 1878.
Linnean Society—Journal, Nos. 122, 123, 124. 8vo. 1882.
Lisbon, Sociedade de Geographia—Bulletin, 3e Serie, No. 5. 8vo. 1882.
Mann, Robert James, M.D. F.R.C.S. M.R.I. (the Author)—Familiar Lectures on the Physiology of Food and Drink. 12mo. 1882.
Marcet, William, M.D. F.R.S. (the Author)—Southern and Swiss Health Resorts. 12mo. 1883.
McCosh, John, M.D. (the Author)—Sketches in Verse, and the War of the Nile. 16mo. 1882.

Mechanical Engineers' Institution—Proceedings, Nos. 3, 4. 8vo. 1882.
Meteorological Office—Hourly Readings, 1874–1880. fol.
Hourly Readings, 1881. Part II. 4to. 1882.
Report of the Meteorological Council of the Royal Society to 31st March, 1882. 8vo. 1882.
Meteorological Society—Quarterly Journal, No. 44. 8vo. 1882.
The Meteorological Record, Nos. 6, 7. 8vo. 1882.
National Association for Social Science—Proceedings, Vol. XVI. Nos. 1, 2. 8vo. 1882.
Norwegischen Commission der Europäischen Gradmessung—Geodatische Arbeiten. Heft 1, 2, 3. 4to. 1882.
Vandstandsobservationer. Hefte 1. 4to. 1882.
Pharmaceutical Society of Great Britain—Journal, Dec. 1882, Jan. 1883. 8vo.
Photographic Society—Journal, New Series, Vol. VII. Nos. 2, 3, 4. 8vo. 1882.
Pritchard, H. Baden, Esq. F.C.S. (the Editor)—Year Book of Photography for 1883. 16mo. 1882.
Purdy, Frederick, Esq. F.S.S. M.R.I.—Local Taxation Returns, 1880–1. fol. 1882.
Annual Report of the Local Government Board. Supplement. Report of Medical Officer, 1879. 8vo. 1880.
Royal Society of London—Proceedings, No. 222. 8vo. 1882.
Russell, Hon. F. A. Rollo, M.R.I. (the Author)—The Improvement of Climate with Slight Elevation. 8vo. 1882.
Saxon Society of Sciences, Royal—Philologisch-historische Classe: Abhandlungen: Band VIII. No. 4. 8vo. 1882.
Verhandlungen, 1881, Nos. 1, 2. 8vo. 1882.
Mathematisch-physische Classe: Abhandlungen: Band XII. Nos. 7, 8. 8vo. 1881–2.
Verhandlungen, 1881, Nos. 1, 2. 8vo. 1882.
Seismological Society of Japan—Transactions, Vols. I.–IV. 8vo. 1880–2.
Simon, Collyns, Esq. Hon. LL.D. (the Author)—The Solar Illumination of the Solar System. 8vo. 1879.
Smyth, C. Piazzi, Esq. (the Author)—Madeira Spectroscopic. 4to. 1882.
Society of Arts—Journal, Dec. 1882, Jan. 1883. 8vo.
Statistical Society—Journal, Vol. XLV. Part 4. 8vo. 1882.
Symons, G. J.—Monthly Meteorological Magazine, Dec. 1882, Jan. 1883. 8vo.
Telegraph Engineers, Society of—Journal, Vol. XI. No. 44. 8vo. 1882.
United Service Institution, Royal—Journal, No. 118. 8vo. 1882.
United States Coast Survey—Methods and Results of Meteorological Researches, Part 3. 4to. 1882.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1882: Nos. 9, 10. 4to.
Victoria Institute—Journal of Transactions, No. 63. 8vo. 1882.
Yorkshire Archæological and Topographical Association—Journal, Part 28. 8vo. 1882.

WEEKLY EVENING MEETING,

Friday, February 9, 1883.

Henry Pollock, Esq. Manager, in the Chair.

Moncure D. Conway, Esq. M.A.

"*Emerson and his Views of Nature.*"

When the statue of Carlyle was unveiled, the speaker on that occasion (Dr. Tyndall) expressed the hope that some day a memorial of Ralph Waldo Emerson might be placed beside it. The long friendship between those men, which defied dissimilarities and differences to sunder their hearts, was due to their profound moral relationship. They were children of the Human Age of Literature. Indeed Literature is hardly a large enough word to describe the works of men whose words were "half-battles" and victories. The artist of the Carlyle memorial has significantly piled Carlyle's books beneath his chair. For Emerson, however, the intellectual and poetic life and work were precisely those which circumstances made the most practical and humane. Although, by moral and humane aims, the descendant of the Puritans and the descendant of the Covenanters were brothers, the chief influence on the intellect of Emerson was rather that of Wordsworth, whose poetry raised in him the vision of a loving life with nature. When Emerson visited Rydal Mount in 1833, Wordsworth warmly advised him against too much intellectual culture. He may have recognised the fact that Emerson was at that time chiefly interested in the discussions which had followed the controversy between Geoffroy St. Hilaire and Cuvier. Of all that literature which prepared the way for Charles Darwin's great generalisation, in French, German, and English, Emerson was an assiduous student. Perhaps his first lecture was one given in the Winter of 1833–34, on 'The Relation of Man to the Globe.' It has not been published or reported, but Dr. Emerson has explored it for me, and it contains passages showing elation at meeting the dawn of a great truth. "By the study of the globe in very recent times we have become acquainted with a fact the most surprising—I may say, the most sublime—to wit, that man, who stands in the globe so proud and powerful, is no upstart in the creation, but has been prophesied in nature for a thousand, thousand ages before he appeared; that from times incalculably remote there has been a progressive

preparation for him, an effort (as physiologists say,) to produce him: the meaner creatures, the primeval sauri, containing the elements of his structure and pointing at it on every side, whilst the world was at the same time preparing to be habitable by him. He was not made sooner because his house was not ready." "Man is made, the creature who seems a refinement on the form of all who went before him, and more perfect in the image of his Maker by the gift of moral nature; but his limbs are only a more exquisite organization,—say, rather, the finish of the rudimental forms that have been already sweeping the sea and creeping in the mud: the brother of his hand is even now cleaving the Arctic sea in the fin of the whale, and innumerable ages since was pawing the marsh in the flipper of the saurus." It is in a sense studying the law of evolution itself to study the impression it made upon the mind of Emerson, as a law of which he was absolutely convinced in the beginning of his career. His first book *Nature* (1836) is a Vedas of the scientific age, in which instead of man's ancient worship of sun, cloud, star, these glorious objects unite in celebration of Man. The development of man is the spiritualization of nature. The course of man's culture turns nature to a kingdom of Use, translates its laws into ethics, its aspects into language, its facts and phenomena into science, builds its sublimities into a temple. Man is what nature means. The only break in the radiant optimism of the book is a complaint that Science had not explained the relationship of man to the forms around him, the unity of things; and almost at the very moment when that book appeared (Sept., 1836) young Charles Darwin landed from the *Beagle* with tidings of the new intellectual world for which the new world thinker was calling. It was to be twenty-two years yet before Darwin was prepared to announce his theory, but meanwhile Emerson had gained some farther light in that direction. It came to him while exploring an unpromising region,—the works of John Hunter. In an essay he speaks of an "electric word" of Hunter's on development. I can find but one reference to development in Hunter's works. Palmer's Hunter appeared in 1835, while Emerson was writing his *Nature*, and its reference to development is in Vol. I., a footnote to p. 265:—"If we were capable of following the progress of increase of number of the parts of the most perfect animal, as they formed in succession, from the very first to its state of full perfection, we should probably be able to compare it to some of the incomplete animals themselves of every order of animals in the creation, being at no stage different from some of those inferior orders; or in other words, if we were to take a series of animals, from the more imperfect to the perfect, we should probably find an imperfect animal corresponding with some stage of the most perfect."

The fact that each animal passes in the course of its development through stages comparable to those of adult animals of lower organization is now explained by evolution; to Emerson it was itself a partial explanation, bringing order into phenomena which traditional theories

left chaotic. His second essay on Nature (1844) shows him realising how vast may be the function of a small agency working in boundless time and boundless space. Geology, he says, has taught us to disuse our dame-school measures. "We knew nothing rightly for want of perspective. Now we learn what patient periods must round themselves before the rock is formed, then before the rock is broken, and the first lichen race has disintegrated the thinnest external plate into soil, and opened the door for the remote flora, Ceres and Pomona, to come in. How far off yet is trilobite! how far the quadruped! how inconceivably remote is man! All duly arrive, and then race after race of men. It is a long way from granite to oyster; farther yet to Plato and the preaching of the immortality of the soul. Yet all must come as surely as the atom has two sides." Simultaneously with the appearance of this essay in America, the 'Vestiges of Creation' appeared in England. Agassiz sought to persuade Emerson that these relations and degrees of forms were only ideal; but Emerson's idealism was too wide to admit of any dualism in nature. In the order of thought he read the order of nature, before it was proved. "Development" was the religion of Emerson before it was the discovery of science. It was the vision of his poetic genius, the affirmation of his moral enthusiasm, the hope of his humanity. He founded his life and work upon it long before Darwin proved that he had founded on a rock. Whenever he touched the theme he broke forth into song. In his poem "Musketaquid" his view of natural evolution is exquisitely humanised.

The charm which Emerson's writings have for scientific men is partly due to the nature in them; but also to the fact that in them is foreshadowed the kind of character, sentiment, religion, legitimately related to the scientific generalisations which have alarmed many worthy people, not unnaturally solicitous for the spiritual beauty of life. When others were alarmed at this or that new statement, Emerson said: "Fear not the new generalisation. Does the fact look crass and material, threatening to degrade thy theory of Spirit? Resist it not; it goes to refine thy theory of matter just as much." This is from his 'First Series of Essays,' a volume which closes with this pregnant sentence:—"When Science is learned in love, and its powers are wielded by love, they will appear the supplements and continuations of the material creation." Whatever his audience, Emerson always did his best; he never put out his talent to work for him, reserving his genius. In America Emerson's life and spirit were always the strongest argument on the progressive side. When his house was burned down, in 1872, persons of different parties and beliefs insisted, despite his deprecations, on rebuilding what had been a home for many minds.

[M. D. C.]

WEEKLY EVENING MEETING,

Friday, February 16, 1883.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Professor William C. Williamson, LL.D. F.R.S.

On some Anomalous Oolitic and Palæozoic forms of Vegetation.

In the course of lectures which I have delivered in this hall during the last few weeks I have tried to show the kind and amount of support which the study of fossil vegetation renders to the Darwinian doctrine of Evolution. With such an aim, attention was necessarily limited to plants whose true botanical affinities appeared to me to be virtually indisputable. This limitation was indispensable, since such plants alone could be admitted as evidence when the question of the pedigree of the vegetable kingdom was in question. Plants whose organisation was obscure, and whose external forms might indicate, with equal probability, relationship to more than one amongst the very different types which appeared during the later developments of the vegetable kingdom, could not be relied upon as witnesses testifying to the facts which actually occurred as ages rolled by.

Unfortunately the Palæozoic and Mesozoic strata have furnished a considerable number of such undetermined plants. The history of the study of many of these is but too humiliating to scientific men. A considerable number of objects which, beyond doubt, were not vegetables have had places assigned to them in the history of the plant race; whilst other very similar, but equally indeterminable forms, may not only have been plants, but as such may have played a very important part in the genetic chain of vegetable life. Such objects must have had an ancestry, and may have had important descendants. Yet, owing to their obscure indications, we can assign to them no position in the story of vegetable ontogeny. It is to a few striking examples of these doubtful objects that I propose calling your attention to-night.

In Plate II. Fig. 6, and Plate III. Fig. 7, of Young and Bird's 'Geological Survey of the Yorkshire Coast,' published in 1822, two specimens of fossil plants are represented. The former of these specimens was regarded by the authors as the fruit of a Cycadean plant, the leaves of which occur in the stratum in which the specimen was found, in great abundance. This stratum is a ferruginous sandstone, one of the subdivisions of the Inferior oolite seen at Runswick Bay and other parts of the sea-coast of north Yorkshire. I obtained numerous specimens of the same objects in 1832 and subsequent years, and I had the opportunity of examining others in the museums

of Scarborough, Whitby, and York. The result of these and other studies was a memoir written more than thirty years ago, but not published until 1868.* The conclusions at which I then arrived were that the two very different objects on the table before us were possibly the male and female reproductive organs of some Diœcious plant; and from their apparently constant association in the Oolitic sandstone of Saltwick and Hawsker with the fronds and stems of the Cycad, *Zamia gigas*, I said "the inference that they are parts of one plant, though incapable of proof, is forcibly suggested," "at all events such suggestions will raise definite points for future investigation."†

Fig. 1 represents the general aspect of what I believed to be the Andrœcium or male organ. The globular structures *a*, *a* are composed of a circular series of narrow, curved bracts, enclosing a peculiar axis. Fig. 2 represents a section through the centre of this organism, in which *a*, *a* are these bracts; *b* is the central pyriform axis, the greater part of which is invested by a cortical layer, *c*, of long narrow tubes or cells, disposed vertically to the surface of the organ. The peduncle (Figs. 1, *d*; 2, *d*), which was sometimes branched, as in Fig. 1, was clothed with shorter overlapping leaves or bracts (Figs. 1 and 2, *e*). The supposition that the cortical layer (Fig. 2, *c*) possibly bare antheridial organs, was rather inferred from the structure of the specimen represented diagramatically in Fig. 3. This figure represents a section through the middle of a verticil of incurved bracts, *a*, which have coalesced at their bases into a cup-shaped peduncle *b*. This organism has never yet been found attached to any other; but what led me to infer that it was possibly a gynœcium or female structure, was the aspect of the upper surface of the free portion of each bract, which has the appearance seen in Fig. 4. The raised central ridge *a* is arrested at *b* by a pair of oblong depressions, which seemed so obviously adapted for bearing a pair of Cycadean ovules, that the possibility of such having been their possible function could not well be overlooked. Still lower down (Fig. 3, *c*) are pairs of little circular depressions arranged in parallel rows, which seem to indicate the former position of other unknown organs.

In a memoir, published in the 'Linnean Transactions' along with mine, Mr. Carruthers proposed for the group of fossil Cycads to which my plants appeared to belong, the generic name of Williamsonia; and as the Cycad, whose stems and fronds were associated with my fossils, was the *Zamia gigas* of authors, this plant, along with my fossils, stood in his memoir as *Williamsonia gigas*, the type representative of the new genus.

Since the publication of the above memoirs, new interest has been given to these curious fossils by the discovery that, whatever may be their botanical affinities, they represent a type that has been widely

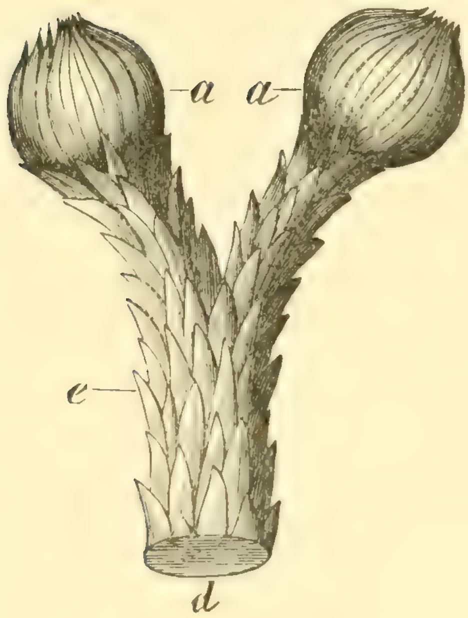
---

* 'Contributions towards the History of *Zamia gigas*, by W. C. Williamson, F.R.S.' 'Transactions of the Linnean Society,' vol. xxvi. p. 663.

† Loc. cit., p. 672.

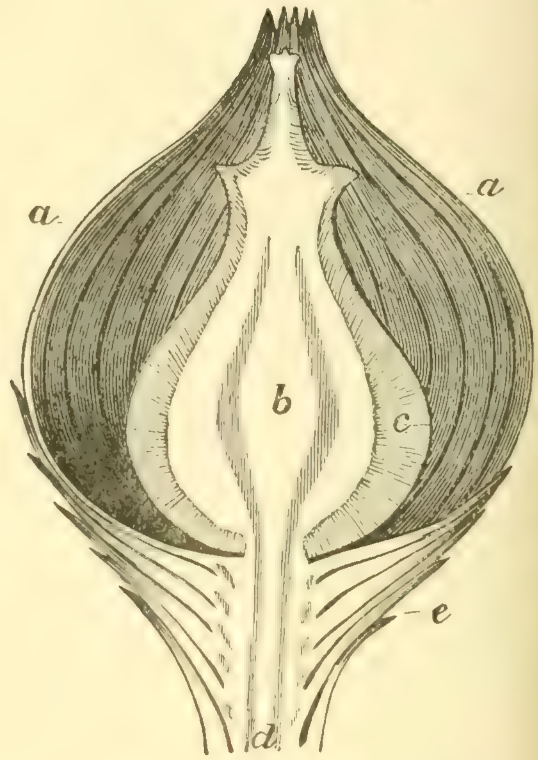
diffused during the Oolitic period. Similar reproductive organisms, though of very distinct species, have been met with in the Oolites of

FIG. 1.



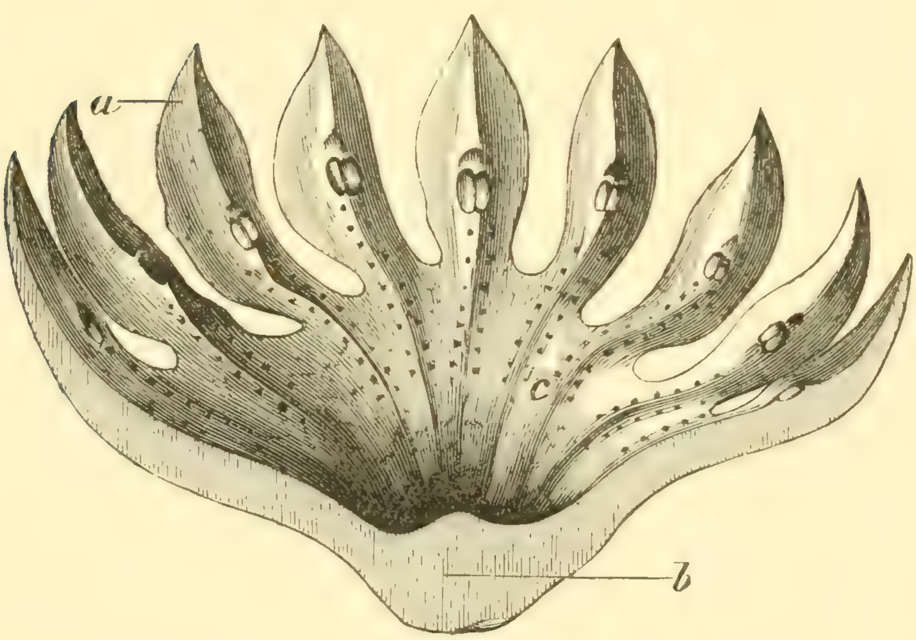
Restored branching peduncle of *Williamsonia gigas.*

FIG. 2.



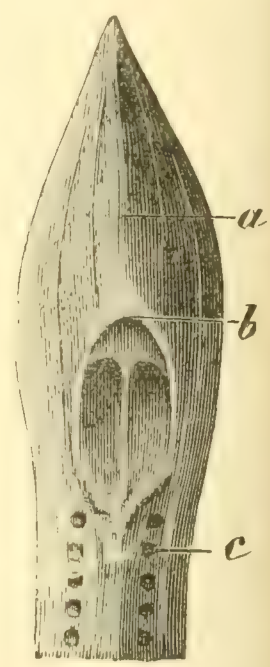
Vertical section through the supposed Andrœcium of *Williamsonia gigas.*

FIG. 3.



Restored half-section of the supposed gynœcium of *Williamsonia gigas.*

FIG. 4.



India, France, and some of the Baltic provinces; and since some of these have been found apart from any associated Cycadean remains, reasonable doubts have arisen as to their Cycadean character. The

immediate result has been the limitation of the genus Williamsonia to these reproductive organisms, and the restoration of the name *Zamia gigas* to the stems and fronds to which it was originally applied, and respecting the Cycadean character of which there is no doubt.

What, then, are *Williamsonia gigas* and its allies? Dr. Nathorst of Stockholm has published a memoir,* in which he suggests that it has belonged to the curious fungoid-looking order of the Balanophoræ. This suggestion I am unable to adopt. An inspection of the shapeless and shrivelled Balanophoræ, seen in any herbarium, will suffice to show the improbability of their having ever been fossilized into the elegant and sharply defined forms of the *Williamsonia gigas*. The Marquis of Saporta is inclined to believe that it has been the inflorescence of a spadicifloral Monocotyledon. This supposition seems to me quite as devoid of probability as that of M. Nathorst. The *texture* of its foliar organs still appears to me suggestive of an abundant supply of the sclerenchyma, which produces the firmness, almost rigidity, so characteristic of the foliage of the Cycads; and I am far from certain that even yet its nearest relatives will not be found in that group. Anyhow, for the present we can only conclude that, whilst Williamsonia represents an undetermined form of vegetation, the importance of the genus is alike evident from its morphological peculiarities and its wide geographical diffusion.

I have long possessed this specimen of a curious stem from the beds in which *Williamsonia gigas* occurs, and which has been briefly described by Sir Charles Bunbury under the name of *Calamites Beanii*.† The occurrence of a true Calamite in the Oolitic rocks would be a palæontological fact of considerable importance; but I cannot admit the Calamitean character of the plant so designated. Indeed Sir Charles Bunbury kindly informs me that he has no strong convictions about its Calamitean nature. So far as external appearances are concerned, it more closely resembles the stem of one of the arborescent Gramineæ. But such appearances have very little Taxonomic value. Nevertheless, the plant stands out in prominent distinctiveness from amongst the Ferns, Cycads, and Conifers that grew around it, forcibly suggesting the idea of an arborescent Monocotyledon; and if such has been its character, its position amongst these older Oolites would make it, if not the earliest, one of the earliest representatives of the Monocotyledonous group. In that case it represents a link in the ontological chain of vegetable life of great importance, though one of which at present we can make no use.

Though we have no great difficulty in determining what are and what are not ferns, the difficulty is often insuperable when we try to ascertain with which of the varied fern-types any fossil form presents

* Ofversigt af Kongl-vetenskaps-Akademiens Förhandlingar, 1880, : 0 Stockholm.

† 'Quarterly Journal of the Geological Society of London,' vol. vii. (1851) p. 189.

the strongest affinities. This difficulty is especially felt when studying the large, ill-defined genera, Pecopteris, Neuropteris, and Sphenopteris. Three examples may be selected from the latter genus in illustration of the worthlessness of our present classification of these objects.

In 1837 I figured and described, in the 'Fossil Flora of Great Britain,'* the first discovered examples of the Yorkshire Oolitic genus Tympanophora. Their nature was then wholly problematical; but in 1844 † I discovered that the *Tympanophora racemora* was but a sporangial pinnule of the *Pecopteris Murrayana* of Brongniart, the *Sphenopteris Murrayana* of Phillips. I have now before me another Sphenopteris from the Yorkshire coast, apparently the *Sphenopteris hymenophylloides* (*S. stipata* of Phillips), in which the fertile pinnules are also Tympanophoræ. Fig. 5A represents the sterile and Fig. 5B the fertile forms of this plant. Brongniart compares this sporangial fructification with that of the arborescent Thyrsopteris; but it may with equal propriety be compared with that of several of the Davallias or Hare's-foot ferns. But a very different form of Sphenopteris

FIG. 5A.

Sterile leaflets of *Sphenopteris hymenophylloides.*

FIG. 5B.

Fertile leaflets of *Sphenopteris hymenophylloides.*

has just been described by Mr. Robert Kidson of Stirling, under the name of *Sphenopteris tenella* of Brongniart, but which latter, he informs me, he now regards as a synonym of Gutbier's *Sphenopteris lanceolata*, the older name. I am indebted to Mr. Kidson for specimens of this plant with the sporangia of the fertile fronds in an exquisite state of preservation. As is so often the case with the spore-

* Vol. iii. pl. 170.

† Brongniart's 'Tableau des genres de Vegetaux fossiles,' p. 46.

bearing ferns of the Palæozoic strata, these spore-bearing fronds are reduced, by the non-development of the cellular parenchyma, to little more than the skeletonised venation; and it is so in this instance. As Mr. Kidson has shown in his memoir,* the sporangia are oval (Fig. 6A, *b*), and planted on the rachis (Fig. 6A, *a*) or midrib of the pinnule in two parallel rows, those of each row alternating with those of the adjoining one. No annulus is present, but there is a single terminal orifice (Fig. 6A, *c*), through which the imprisoned spores have escaped. This form of sporangium is identical in all essential features with that of the recent Danæa and the fossil Danæopsis, in

FIG. 6A.

Portions of two fertile leaflets of *Sphenopteris lanceolata.*

FIG. 6B.

Sterile leaflets of the same.

which, also, each series consists of two rows of sporangia, arranged in alternating order. Fig. 6B represents a fragment of the sterile frond. This combination of a Sphenopterid frond with the sporangia of a Danæa is wholly unknown at the present day. Many other equally remarkable combinations occur amongst the Palæozoic ferns.

The Carboniferous strata have furnished several very remarkable stems, in which the internal structure is perfectly preserved, but to the systematic relations of which we have failed to obtain any clue. One of the most striking of these is that which I have described under the name of *Lyginodendron Oldhamium*,† of which we have not only obtained small twigs, but stems, and such numerous casts of the exteriors of stems as prove it to have been a tree of large dimensions, and abundantly distributed over wide areas; yet we know nothing either of its foliage or of its botanical affinities. Fig. 7 represents an

* 'On the fructification of the *Eusphenopteris tenella* and *Sphenopteris microcarpa*,' Royal Physical Society, Edinburgh, April 19th, 1882.

† 'Organisation of the Plants of the Coal Measures.' 'Phil. Trans.' 1873, pls. 22-6.

outline of a transverse section of a young branch less than an inch in diameter. We have a cellular pith, *a*, enclosed within a vascular zone. The latter consists of detached clusters of vessels at *b*, surrounded by a regular exogenous cylinder, *c*, composed of thin, radiating, vascular laminæ, separated by large medullary rays. This zone was surrounded by a cambium layer, through which it obtained additions to its periphery until it became a large, exogenously-developed tree. Externally to the vascular zone was a thick bark, chiefly composed of two layers *g*, *h*. Two other facts of morphological importance may be

Fig. 7.

Diagramatic section of a branch of *Lyginodendron Oldhamium*.

noticed. Four or five pairs of isolated vascular bundles, *e*, pass vertically through the inner bark, close to the periphery of the vascular zone, each pair of which eventually moves obliquely outwards to some unknown, but most probably foliar, appendage. Upon the nature of these appendages ten years of diligent search has failed to throw any light.

The second fact is connected with the vascular zone, *c*, when in its very young state. The vascular bundles, *b*, the vessels of which were not arranged in radiating order, then constituted an almost, if not absolutely unbroken ring, and were surrounded by the equally uninterrupted exogenous zone, *c*. But the general growth of the latter and of the pith was not accompanied by any corresponding growth of this intermediate vascular zone, *b*; hence this became broken up into the detached portions (Fig. 7 *b*) already mentioned. So long as the exogenous cylinder, *c*, retained its integrity, it was surrounded by an equally uninterrupted zone of cambium, the instrument of its exoge-

nous growth. But these regular conditions have frequently been disturbed; first by the cambium ring extending itself partially or wholly around one or both of the free paired bundles, *e*, leading either to a one-sided development of exogenous vascular laminæ on their external sides, as at Fig. 7, *e*′, or sometimes, as in *f*, entirely surrounding a bundle with a complete cylinder of radiating laminæ. But further irregular developments have sometimes taken place. Some of the cellular medullary rays separating the vascular laminæ of the zone *c* have undergone great enlargement, breaking up the zone into several segments of a circle; and in one such example in my cabinet, the cambium has extended itself centripetally round the two converging sides of each vascular segment, and formed a boundary line between its inner angle *b* and the pith *a*. The consequence of this has been the unwonted development of new vascular laminæ, which project centripetally into the pith from the cluster of non-radial vessels *b*.

This tendency to a multiplication of independent centres of exogenous growth within the area of an enclosing bark, inevitably reminds the botanist of the not wholly dissimilar conditions characteristic of some Sapindaceæ, such as Paullinia and Sejania. Only what is a variable feature in the Lyginodendron, is in these latter plants a regular and normal one; but the strong tendency to such peculiar variations in an archaic type may easily be conceived to have led to the ultimate production of more constant differentiated forms.

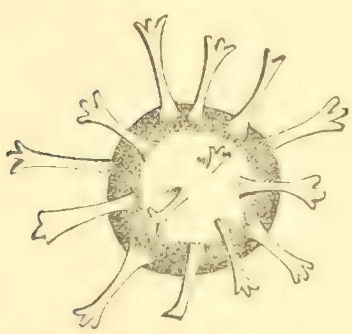
A second remarkable and anomalous stem from Burntisland, in Fifeshire, is described in the same memoir as the Lyginodendron, under the name of *Heterangium Grievii*. This plant must have been sufficiently abundant to have formed a conspicuous object in the Carboniferous forests of that portion of the primæval world; yet we are as ignorant about its foliage, fructification, and botanical relationships as we are of those of the Lyginodendron.

In the 'Philosophical Transactions' for 1878 I described, under the generic name of Asteromyelon, the central portions of the stem and branches of a plant that was very common in the carboniferous area of west Yorkshire, eastern Lancashire, and the intermediate Pennine Hills. The discovery of its curious bark two years ago was made the occasion by Messrs. Cash and Hicks of giving to this plant the specific name of Williamsonia. In the general plan of its organisation this plant has many features in common with that of the living Marsileæ. This is especially the case with the bark; but it exhibits extraordinary variability in the structure of its central axis. Sometimes the actual centre is occupied by a vascular bundle from which cellular elements are almost, if not wholly, excluded. In other cases this centre is occupied by a very large and beautiful pith, transverse sections of which present the exquisite stellate form that led me to give to the object its generic name of Asteromyelon. We have here another example of an organism exhibiting great variations of structure, due partly, no doubt, to differences of function, but partly to difference of surroundings.

Another group of anomalous objects has been found in the coal-measures, to which I have given the provisional name of Sporocarpon. These are small, spherical, hollow, cellular structures, often filled with free-cells, reminding us of many of the reproductive structures of the Rhizocarpous plants. I have little doubt that these are truly reproductive structures, but we have as yet wholly failed to discover to what known fossil plants they belong, or to what order they must be assigned. Yet, as in the case of some other objects to which I have already referred, they are so far from being rare, and their individuality of form is so marked, that they cannot have been unimportant morphological members of the Palæozoic Vegetable Kingdom. They represent links in the scale of life—but we know not what.

The multiplicity of these indeterminable structures is great, and consequently apt to discourage the investigator; at the same time he has his encouragements, since some that have long been equally obscure have at length rewarded persevering research by yielding up their secrets. Two instances may be quoted as examples of forms with which we have long been familiar, but whose true nature has been ascertained but recently. In the first of these our success has been but partial. I long ago discovered numerous small spherical objects, especially in the lower Carboniferous beds at Halifax, to which I assigned the name of Zygosporites (Fig. 8): similar objects appear to have been met with in France, and were believed by one French naturalist to be Zygospores of the well-known aquatic living group of the Desmideæ. I saw no sufficient reason for recognising the existence of Desmids during the Carboniferous age. The problem has been solved by the recent discovery of these objects in the interior of a true Sporangium, proving them to be the spores of some Cryptogamic plant very different from a Desmid. Thus much is finally settled. But we may venture further and say that they are the spores of a Strobilus, which I some time ago described under the name of *Volkmannia Dawsoni*. We have yet to learn with certainty to what plant this Strobilus has belonged. The probabilities are that it was Asterophyllitean.

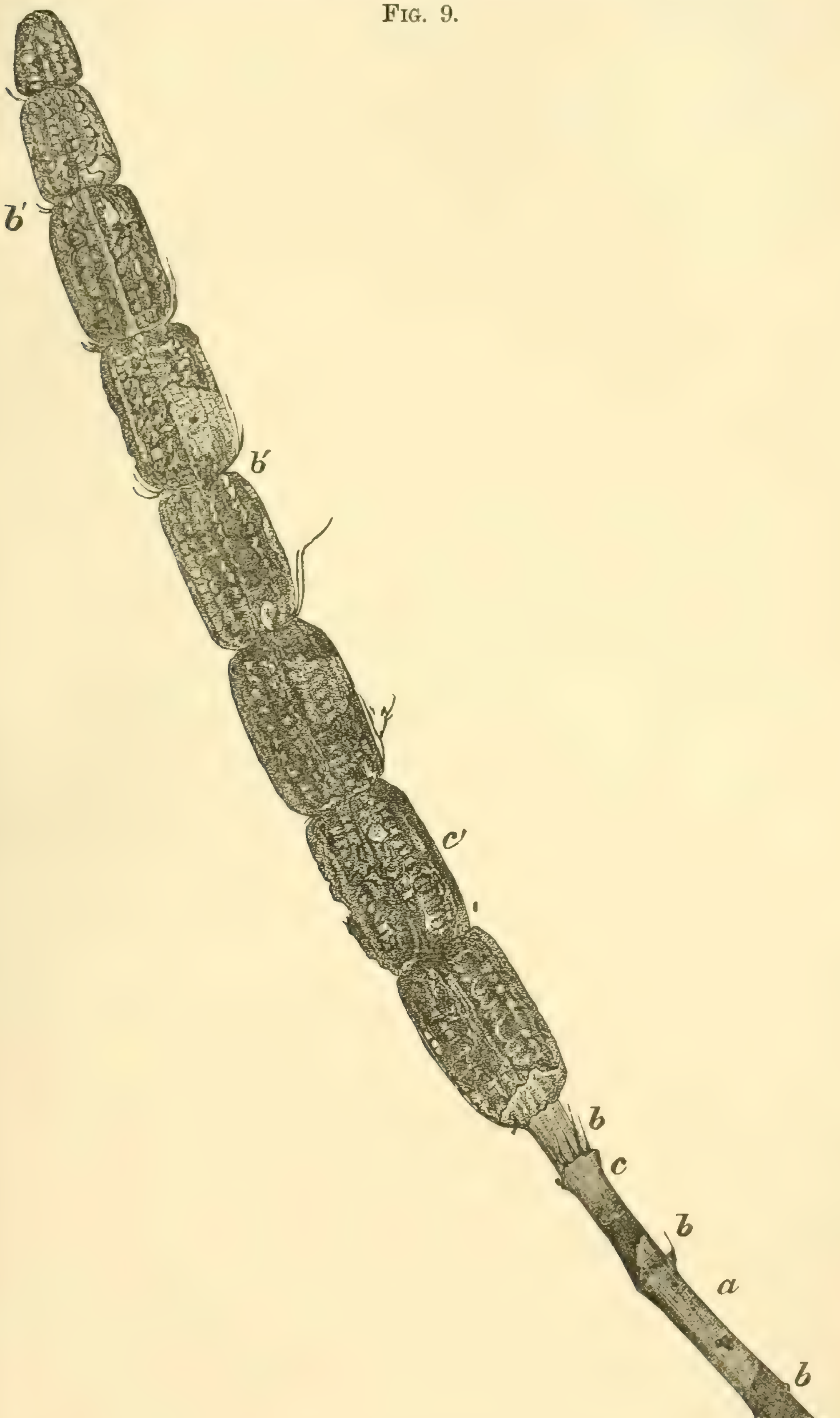
Fig. 8.



My second instance is still more important in its bearing upon Palæontological Darwinism. Some years ago a very remarkable specimen was discovered in the lower coal-measures at Granton, near Edinburgh, to which the name of *Pothosites Grantoni* was given, and which has been quoted by Sir Charles Lyell and others * as a flowering plant. Lyell says, "The fossil has been referred to the Aroidiæ, and there is every probability that it is a true member of this order. There can at least be no doubt as to the high grade of its organisation,

* 'Students' Elements of Geology,' p. 424. Balfour's 'Palæontological Botany,' p. 66.

FIG. 9.



Pothosites Grantoni.

and that it belongs to the Monocotyledons." I long ago expressed my strong doubts as to the correctness of this assertion.* On a recent visit to the Museum of the Glasgow University, my friend, Mr. John Young, showed me one impression of a newly discovered specimen of the same plant which fully justified my scepticism as to its Phanerogamous nature. The opposite and more perfect impression was in the hands of Mr. Robert Kidson, who has since described this and other examples in a paper now passing through the press, but who has not only provided me with impressions of the yet unpublished plates, but has kindly allowed me to copy his figure of the Glasgow specimen in the woodcut (Fig. 9). The fossil is evidently the fructification of a plant of the Asterophyllitean type, having a jointed stem, *a*, giving off verticils of short leaves, not only at the nodes of the nude stem (*b*), but at those (*b′*, *b′*) of the spore-bearing part of the branch.† Unfortunately none of the specimens of Pothosites hitherto discovered retain any traces of the internal organisation of their stems. But there is not much difficulty in determining the general features of this organism. The constrictions, *b′*, are obviously nodes like those of the naked stem *b*, hence the thickened joints, *c′*, correspond to the internodes *c*. Now none of these Asterophyllitean plants give off organs of fructification from the internodes. They always spring directly or indirectly from the nodes. Each joint, *c′*, of Fig. 9 is made up of longitudinal lines of Sporangia which can have no organic union with the stem which they invest, save at the node immediately below each joint. In the hitherto known forms, the Sporangia spring directly or mediately from the axils of separate leaves; but they are frequently supported on separate small branches, which spring in verticils from the node of some larger stem. I surmise that such is typically the case in the Pothosites; only instead of each secondary branch constituting a distinct, free strobilus, as in Volkmannia and other allied forms, these strobili have here coalesced laterally, forming a cylindrical investment of each internode of the axis—as the free anthers of an ordinary flower have, in the Compositæ, coalesced into a cylinder enclosing the pistil. This explanation seems to me to indicate the true place of the Pothosites amongst the Verticillate-leaved plants of the coal-measures. At any rate we learn how great would have been the mistake of any framer of floral genealogies who, trusting to the authority of text-books, made the Monocotyledons originate with

* 'Essays and Addresses, by Professors and Lecturers of the Owens College, Manchester,' pp. 129–30, 1874.

† Mr. Kidson thinks that the plant is the fructification of the genus Bornea. I am not, however, sure that it can yet be identified with that obscure genus. At present the entire group of these verticillate-leaved Carboniferous plants is in a state of hopeless confusion, from which I fear we shall be long in escaping, since knowledge of the internal structure alone will enable us to determine the types of stem to which they severally belong. At present we are only familiar with those of Calamites, Asterophyllites, and Sphenophyllum, and cannot even identify the leaves of the first two of these genera when detached from such parent stems as retain their internal structures.

Pothosites during the Carboniferous age. It has followed the genus Palmacites, now proved to be a Fern, and no other known trace of any Angiosperm remains associated with the Palæozoic rocks.

I have already referred briefly to one more difficulty that interferes with all attempts to demonstrate in detail the evolution of the vegetable kingdom: I allude to the numerous objects that have been named and classified as Algæ: Of the Algoid character of some, possibly of many, of these forms there can be no doubt, but unfortunately the unquestionable examples are the more recent ones, which have little or no bearing upon the Darwinian problem. That some form of marine vegetation must have existed prior to the advent of the Phytophagous molluscs and other animals whose remains abound in the Cambrian rocks, is too obvious to require further proof than that which is supplied by the existence of such animals. Hence when we find in these Palæozoic rocks objects that *look* like Fucoids, there is an *a priori* probability in favour of their being so. But the detailed construction of a demonstration like that just referred to demands more than this. In assigning to each object its actual place in the ontological history we must have a fair approximation to certainty as to the organisation and Taxonomic position of such objects. But this is exactly what we fail to obtain in the case of these so-called Palæozoic Fucoids. Dr. Nathorst has recently advanced strong evidence supporting his conclusion that many of these objects are merely inorganic casts of the tracks of various animals that have crawled over the plastic sea-bed. On the other hand, many have already been identified as imperfectly preserved branches of plants of comparatively high organisation. Hence we must regard with extreme caution, all attempts to press these dubious fragments into the service of evolution. But the temptation for the evolutionist to do so is a strong one, since the doctrine demands the prevalence, during primæval times, of such lower forms of plant life.

At present we have one fact of the needed kind which seems to be correctly interpreted, viz. that a true Fungus of a low order existed during the middle period of the Carboniferous age. The *Peronosporites antiquius*, from the lower coal-measures of Halifax, seems to be an indisputable Fungus.

The general conclusions at which our present knowledge justifies our arrival seem to be these:—The Palæozoic rocks have hitherto furnished no traces of plants of a higher organisation than that of the Gymnosperms, and even these latter are not of the most developed types; the vast mass of the coæval vegetation is Cryptogamic. The advent of true Phanerogams is at present only proved to have taken place during the Oolitic period. So far, these general phenomena are such as the evolutionist would demand. But, receding into the remoter past, we obtain no further information. These Gymnosperms existed not only during the Carboniferous period, but were as highly organised during the Devonian age as when the coal-beds were accumulated. Here we again stop. We may ask, what were the

ancestors of these ancient Dadoxylons, and the Cycadean plants that grew by their side, along with Ferns, Lepidodendra, Calamites, and other Palæozoic forms of vegetation? The scanty fragments obtained from the Silurian rocks give us no trustworthy answer to this question—scarcely an *ignis fatuus* glimmers before our eyes.

It is further obvious that the numerous plants of uncertain affinities, some of which have formed the special subjects of this lecture, must have played an important part in the Ontogeny of the Palæozoic flora. Though as I have shown, we already know much of the internal organisation of these plants, we cannot at present assign to them their true botanical positions. How much less can we assign that position to plants of which we only know obscure external forms, which rarely can be implicitly relied upon, apart from organisation. Yet a philosophic constructor of a genealogical tree of the vegetable kingdom cannot ignore all these objects. The time has not yet arrived for the appointment of a botanical King-at-arms and constructor of pedigrees.

Whilst so many problems connected with the Palæozoic flora remain unsolved, one fact may be regarded as established indisputably. The forest scenery of the Carboniferous and Devonian ages must have been monotonous and gloomy. The woodland expanse may have displayed many varied and graceful forms. The uplifted stems and feathery foliage of the Calamitean plants may have been projected against the rounded outlines and drooping branches of the giant Lepidodendra. The lowland forests may have been bounded by groups of pine-like Dadoxylons flourishing upon the higher and drier hills; but hill and dale would equally lack the gorgeous colouring supplied by the floral world. Viewed from a distance, the scene may have resembled a tropical landscape of the present time, seen under similar conditions. Though the traveller, penetrating the shady recesses of a tropical forest, occasionally comes upon some small oasis gay with flowers, such displays are too few and too isolated to relieve the monotonous green of the outspread landscape. The tropics have no lines of fragrant hawthorns, whose masses of snowy blossom give light and beauty to the scene. Summer there weaves no carpet clothing meadow and pasture with yellow buttercups and "wee crimson tipped" flowers. Autumn makes no upland slopes reflect back the golden hues of the furze, shining in richest yet most harmonious contrast with the heather's purple bloom. Such widespread glories, absent from the equatorial zone, now belong to our own more favoured climes. In Palæozoic times, they were lacking from every portion of the primæval world.

[W. C. W.]

WEEKLY EVENING MEETING,

Friday, February 23, 1883.

Sir Frederick Pollock, Bart. M.A. Vice-President, in the Chair.

Walter Herries Pollock, Esq. M.A.

Sir Francis Drake.

The speaker, in a brief biographical sketch, included a defence and eulogium of Sir Francis Drake, based upon authentic evidence, manuscript and printed, from which many extracts were given. Mr. Pollock confuted the erroneous opinion that Drake was of mean parentage in the modern sense, a notion due, as Dr. Drake has shown, to Camden's Latin word "mediocris" being then rightly translated "mean"—i. e. middling. Drake was proved to be not a mere freebooter, but a man who loved his country, and who hated Spain with the Protestant hatred of the time, as well as for his own private wrongs. His first voyage was with Hawkins, in 1568, when he shared in the defeat in the Bay of Mexico. Of his great voyage, known as "the World Encompassed," begun in November, 1577, many interesting details were given. The conspiracy and punishment of Thomas Doughty, one of the captains, was dwelt on at some length on account of its most important relation to Drake's moral character, and because it has served as a convenient handle for those who are disposed to dismiss Drake and his compeers with the words "Pirates all." The evidence, when considered on all sides, led to the inevitable conclusion that the execution was an act of stern but necessary justice. Doughty confessed his crime, and immediately before his death took the sacrament with Drake and his other judges. Among many interesting facts respecting Drake, Mr. Pollock referred to his great engineering skill in supplying Plymouth with an abundance of fresh water by a channel locally called "The Leat," brought from the confines of Dartmoor, a distance of twenty-four miles. This was an incalculable benefit to the town and to the fleet. Remarks were made on Drake's "singeing the King of Spain's beard" at Cadiz, and on the defeat and dispersion of the Spanish Armada. Mr. Pollock quoted as a specimen of Drake's character, as expressed by his style in writing his answer to the false reports as to the Armada, set about by the Spaniards. "They were not ashamed to publish in sundry languages in print great victories in words, which they pretended to have obtained against this realm, and spread the same in a most false sort over all parts of France, Italy, and elsewhere; when shortly afterwards it was happily manifested in very deed to all nations how their navy, which they termed invincible, consisting of one hundred and forty sail of ships, not only of their own kingdom,

but strengthened with the greatest Argosies, Portugal carracks, Florentines, and large hulks of other countries, were by thirty of Her Majesty's own ships of war and a few of our merchants, by the wise, valiant, and advantageous conduct of the Lord Charles Howard, High Admiral of England, beaten and shuffled together, even from the Lizard in Cornwall, first to Portland, where they shamefully left Don Pedro de Valdez with his mighty ship; from Portland to Calais, where they lost Hugh de Moncado with the galleys of which he was captain; and from Calais, driven with squibs from their anchors, were chased out of the sight of England, some about Scotland and Ireland, where for the sympathy of their religion, hoping to find succour and assistance, a great part of them were crushed against the rocks, and those other that landed, being very many in number, were, notwithstanding, broken, slain, and taken; and so sent from village to village coupled in halters, to be shipped into England, where Her Majesty, of her princely and invincible disposition, disdaining to put them to death, and scorning either to retain or entertain them, they were all sent back again to their countries, to witness and recount the achievement of their invincible and dreadful navy. Of which the number of soldiers, the fearful burthen of their ships, the commanders' names of every squadron, with all other magazines of provisions, were put in print as an army and navy irresistible and disdaining prevention, with all which their great terrible ostentation they did not in all their sailing round about England so much as sink or take one ship, bark, pinnace, or cockboat of ours, or even burn so much as one sheepcote on this land." Drake sailed, with Sir John Hawkins, on a last and disastrous voyage to South America, which closed the careers of both the Commanders. Hawkins died on Nov. 12, 1595; and Drake died of a dysentery—it is said also of a broken heart—on Jan. 28, 1596. The discourse concluded with an eloquent summary of the fine qualities of our great naval hero.

[W. H. P.]

WEEKLY EVENING MEETING,

Friday, March 2, 1883.

William Bowman, Esq. LL.D. F.R.S. Honorary Secretary and Vice-President, in the Chair.

C. Vernon Boys, Esq.

Meters for Power and Electricity.

The subject of this evening's discourse:—"Meters for Power and Electricity," is unfortunately, from a lecturer's point of view, one of extreme difficulty; for it is impossible to fully describe any single instrument of the class without diving into technical and mathematical niceties which this audience might well consider more scientific than entertaining. If then, in my endeavour to explain these instruments and the purposes which they are intended to fulfil, in language as simple and as untechnical as possible, I am not as successful as you have a right to expect, I must ask you to lay some of the blame on my subject and not all on myself.

I shall at once explain what I mean by the term "meter," and I shall take the flow of water in a trough as an illustration of my meaning. If we hang in a trough a weighted board, then, when the water flows past it, the board will be pushed back: when the current of water is strong, the board will be pushed back a long way: when the current is less it will not be pushed so far: when the water runs the other way the board will be pushed the other way. So by observing the position of the board, we can tell how strong the current of water is at any time. Now suppose we wish to know, not how strong the current of water is at this time or at that, but how much water altogether has passed through the trough *during* any time, as for instance, one hour. Then, if we have no better instrument than the weighted board, it will be necessary to observe its position continuously, to keep an exact record of the corresponding rates at which the water is passing, every minute, or better every second, and to add up all the values obtained. This would, of course, be a very troublesome process. There is another kind of instrument which may be used to measure the flow of the water:—a paddle-wheel or screw. When the water is flowing rapidly the wheel will turn rapidly, when slowly, the wheel will turn slowly, and when the water flows the other way, the wheel will turn the other way, so that, if we observe how fast the wheel is turning we can tell how fast the water is flowing. If now, we wish to know how much water altogether has passed through the trough, the number of turns of the wheel, which may be shown by a counter, will at once tell us. There are therefore, in the case of water, two kinds of instruments, one which measures *at* a time, and

the other *during* a time. The term meter should be confined to instruments of the second class only.

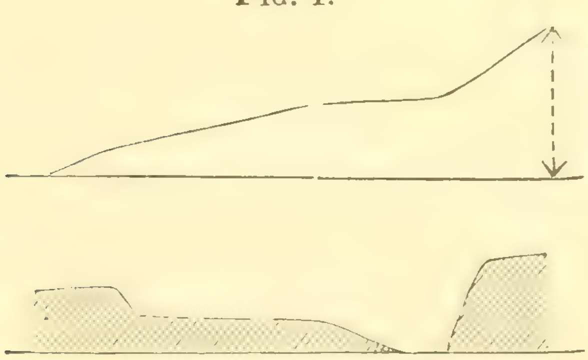
As with water so with electricity, there are two kinds of measuring instruments, one, of which the galvanometer may be taken as a type, which shows by the position of a magnet how strong a current of electricity is *at* a time, and the other, which shows how much electricity has passed *during* any time. Of the first, which are well understood, I shall say nothing; the second, the new electric meters and the corresponding meters for power, are what I have to speak of to-night.

It is hardly necessary for me to mention the object of making electric meters. Every one who has had to pay his gas bill once a quarter probably quite appreciates what the electric meters are going to do, and why they are at the present time attracting so much attention. So soon as you have electricity laid on in your houses, as gas and water is laid on now, so soon will a meter of some sort be necessary in order that the companies which supply the electricity may be able to make out their quarterly bills, and refer complaining customers to the faithful indications of their extravagance in the mysterious cupboard in which the meter is placed.

The urgent necessity for a good meter has called such a host of inventors into the field, that a complete account of their labours is more than any one could hope to give in an hour. Since I am one of this host, I hardly like to pick out those inventions which I consider of value. I cannot describe all, I cannot act as a judge and say these only are worthy of your attention, and I do not think I should be acting fairly if I were to describe my own instruments only and ignore those of every one else. The only way I see out of the difficulty is to speak more particularly about my own work in this direction, and to speak generally on the work of others.

I must now ask you to give your attention for a few minutes to a little abstract geometry. We may represent any changing quantity, as for instance the strength of an electrical current, by a crooked line. For this purpose we must draw a straight line to represent time, and make the distance of each point of the crooked line above the straight line a measure of the strength of the current at the corresponding time. The size of the figure will then measure the quantity of electricity that has passed, for the stronger the current is, the taller the figure will be, and the longer it lasts the longer the figure will be, either cause makes both the quantity of electricity and the size of the figure greater and in the same proportion: so the one is a measure of the other. Now it is not an easy thing to measure the size of a

FIG. 1.



figure, the distance round it tells nothing; there is, however, a geometrical method by which its size may be found. Draw another line, with a great steepness where the figure is tall, and with a less steepness where the height is less, and with no steepness or horizontal where the figure has no height. If this is done accurately, the height to which the new line reaches will measure the size of the figure first drawn: for the taller the figure is, the steeper the hill will be: the longer the figure, the longer the hill: either cause makes both the size of the figure and the height of the hill greater, and in the same proportion: so the one is a measure of the other: and so, moreover, is the height of the hill, which can be measured by a scale, a measure of the quantity of electricity that has passed.

The first instrument that I made, which I have called a "cart" integrator, is a machine which, if the lower figure is traced out, will describe the upper. I will trace a circle, the instrument follows the curious bracket-shaped line that I have already made sufficiently black to be seen at a distance, the height of the new line measures the size of the circle, the instrument has squared the circle. This machine is a thing of mainly theoretical interest, my only object in showing it is to explain the means by which I have developed a practical and automatic instrument of which I shall speak presently. The guiding principle in the cart integrator is a little three-wheeled cart, whose front wheel is controlled by the machine. This, of course, is invisible at a distance, and therefore I have here a large front wheel alone. On moving this along the table, any twisting of its direction instantly causes it to deviate from its straight path; now suppose I do not let it deviate, but compel it to go straight, then at once a great strain is put upon the table which is urged the other way. If the table can move it will instantly do so. A table on rollers is inconvenient as an instrument, let us therefore roll it round into a roller, then on moving the wheel along it the roller will turn and the amount by which it turns will correspond to the height of the second figure drawn by the cart integrator. If, therefore, the wheel is inclined by a magnet under the influence of an electric current, or by any other cause, the whole amount of which we wish to know, then the number of turns of the roller will tell us this amount; or to go back to our water analogy, if we had the weighted board to show current strength, and had not the paddle-wheel to show total quantity, we might use the

Fig. 2.

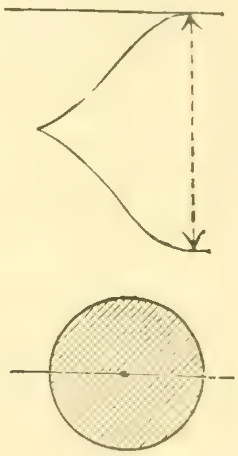
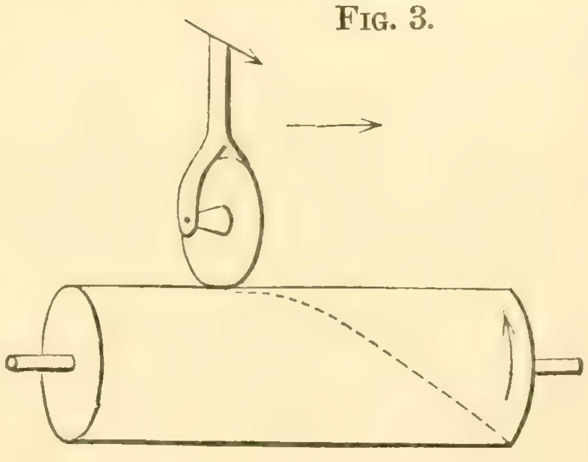


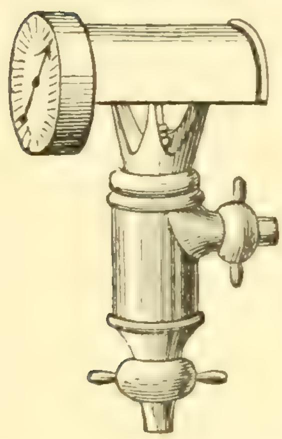
Fig. 3.



board to incline a disc in contact with a roller, and then drag the roller steadily along by clockwork. The number of turns of the roller would give the quantity of water. Instruments that will thus add up continuously indications at a time, and so find amounts during a time, are called integrators.

The most important application that I have made at present of the integrator described, is what I have called an engine-power meter. The instrument is on the table, but as it is far too small to be seen at a distance, I have arranged a large model to illustrate its action.

Fig. 4.



The object of this machine is to measure how much work an engine has done during any time, and show the result on a dial, so that a workman may read it off at once without having to make any calculations.

Before I can explain how work is measured, perhaps I had better say a few words about the meaning of the word "work." Work is done when pressure overcomes resistance, producing motion. Neither motion nor pressure alone is work. The two factors, pressure and motion, must occur together. The work done is found by multiplying the pressure by the distance moved. In an engine, steam pushes the piston first one way then the other, overcomes resistance, and does work. To find this, we must multiply the pressure by the motion at every instant and add all the products together. This is what the engine-power meter does, and it shows the continuously growing result on a dial. When the piston moves it drags the cylinder along, where the steam presses the wheel is inclined. Neither action alone causes the cylinder to turn, but when they occur together the cylinder turns, and the number of turns registered on a dial shows with mathematical accuracy how much work has been done.

In the steam-engine work is done in an alternating manner, and it so happens that this alternating action exactly suits the integrator. Suppose, however, that the action whatever it may be, which we wish to estimate is of a continuous kind, such for instance as the continuous passage of an electric current. Then, if by means of any device, we can suitably incline the wheel, so long as we keep pushing the cylinder along, so long will its rotation measure and indicate the result; but there must come a time when the end of the cylinder is reached. If then we drag it back again, instead of going on adding up, it will begin to take off from the result, and the hands on the dial will go backwards, which is clearly wrong. So long as the current continues so long must the hands on the dial turn in one direction. This effect is obtained in the instrument now on the table, the electric energy meter, in this way. Clockwork causes the cylinder to travel backwards and forwards by means of what is called a mangle motion, but instead of moving always in contact with one wheel, the cylinder goes forward in contact with one and back in contact with another

on its opposite side. In this instrument, the inclination of the wheels is effected by an arrangement of coils of wire, the main current passing through two fixed concentric solenoids, and a shunt current through a great length of fine wire on a movable solenoid, hanging in the space between the others. The movable portion has an equal number of turns in opposite directions, and is therefore unaffected by magnets held near it. The effect of this arrangement is that the energy of the current, that is, the quantity multiplied by the force driving it, or the electrical equivalent of mechanical power, is measured by the slope of the wheels, and the amount of work done by the current during any time, by the number of turns of the cylinder, which is registered on a dial. Professors Ayrton and Perry have devised an instrument which is intended to show the same thing. They make use of a clock, and cause it to go too fast or too slow by the action of the main on the shunt current, the amount of wrongness of the clock, and not the time shown, is said to measure the work done by the current. This method of measuring the electricity by the work it has done, is one which has been proposed to enable the electrical companies to make out their bills.

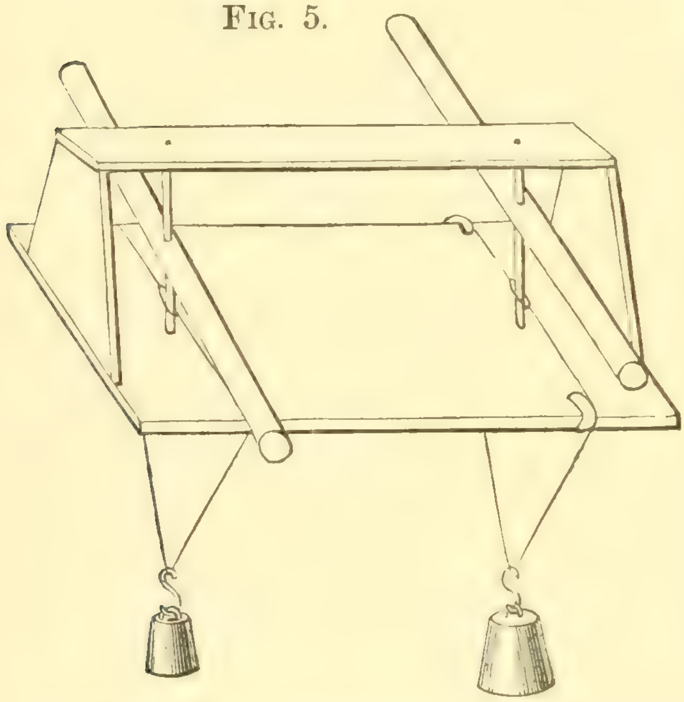
The other method is to measure the amount of electricity that has passed without regard to the work done. There are three lines on which inventors have worked for this purpose. The first, which has been used in every laboratory ever since electricity has been understood, is the chemical method. When electricity passes through a salt solution, it carries metal with it, and deposits it on the plate by which the electricity leaves the liquid. The amount of metal deposited is a measure of the quantity of electricity. Mr. Sprague and Mr. Edison have adopted this method; but as it is impossible to allow the whole of a strong current to pass through a liquid, the current is divided, a small proportion only is allowed to pass through. Provided that the proportion does not vary, and that the metal never has any motions on its own account, the increase in the weight of one of the metal plates measures the quantity of electricity.

The next method depends on the use of some sort of integrating machine, and this being the most obvious method, has been attempted by a large number of inventors. Any machine of this kind is sure to go, and is sure to indicate something, which will be more nearly a measure of the electricity, as the skill of the inventor is greater.

Meters for electricity of the third class are dynamical in their action, and I believe that what I have called the vibrating meter was the first of its class. It is well known that a current passing round iron makes it magnetic. The force which such a magnet exerts is greater when the current is greater, but it is not simply proportional; if the current is twice or three times as strong, the force is four times or nine times as great, or generally, the force is proportional to the square of the current. Again, when a body vibrates under the influence of a controlling force, as a pendulum under the influence of gravity, four times as much force is necessary to make it vibrate twice

as fast, and nine times to make it vibrate three times as fast; or generally, the square of the number measures the force. I will illustrate this by a model. Here are two sticks nicely balanced on points, and drawn into a middle position by pieces of tape to which weights may be hung. They are identical in every respect. I will now hang a 1 lb. weight to each tape, and let the pieces of wood swing. They keep time together absolutely. I will now put 2 lbs. on one tape. It is clear that the corresponding stick is going faster, but certainly not twice as fast. I will now hang on 4 lbs. One stick is going at exactly twice the pace of the other. To make one go three times as fast, it is obviously useless to put on 3 lbs., for it takes four to make it go twice as fast. I will hang on 9 lbs. One now goes exactly three times as fast as the other. I will now put 4 lbs. on the first, and leave the 9 lbs. on the second; the first goes twice while the second goes three times. If instead of a weight we use electro-magnetic force to control the vibrations of a body, then twice the current produces four times the force, four times the force produces twice the rate: three times the current produces nine times the force, nine times the force produces three times the rate, and so on: or the rate is directly proportional to the current strength. There is on the table a working meter made on this principle. I allow the current that passes through to pass also through a galvanometer of special construction, so that you can tell by the position of a spot of light on a scale the strength of the current. At the present time there is no current; the light is on the zero of the scale, the meter is at rest. I now allow a current to pass from a battery of the new Faure-Sellon-Volckmar cells which the Storage Company have kindly lent me for this occasion. The light moves through one division on the scale, and the meter has started. I will ask you to observe its rate of vibration. I will now double the current; this is indicated by the light moving to the end of the second division on the scale: the meter vibrates twice as fast. Now the current is three times as strong, now four times, and so on. You will observe that the position of the spot of light and the rate of vibration always correspond. Every vibration of the meter corresponds to a definite quantity of electricity, and causes a hand on a dial to move on one step. By looking at the dial, we can see how many vibrations there have been, and therefore how much electricity

Fig. 5.

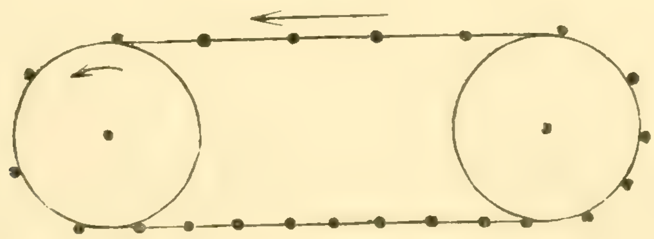


has passed. Just as the vibrating sticks in the model in time come to rest, so the vibrating part of the meter would in time do the same, if it were not kept going by an impulse automatically given to it when required. Also, just as the vibrating sticks can be timed to one another by sliding weights along them, so the vibrating electric meters can be regulated to one another so that all shall indicate the same value for the same current, by changing the position or weight of the bobs attached to the vibrating arm.

The other meter of this class, Dr. Hopkinson's, depends on the fact that centrifugal force is proportional to the square of the angular velocity. He therefore allows a little motor to drive a shaft faster and faster, until centrifugal force overcomes electro-magnetic attraction, when the action of the motor ceases. The number of turns of the motor is a measure of the quantity of electricity that has passed.

I will now pass on to the measurement of power transmitted by belting. The transmission of power by a strap is familiar to every one in a treadle sewing-machine or an ordinary lathe. The driving force depends on the difference in the tightness of the two sides of the belt, and the power transmitted is equal to this difference multiplied by the speed; a power-meter must, therefore, solve this problem—it must subtract the tightness of one side from the tightness of the other side, multiply the difference by the speed at every instant, and add all the products together, continuously representing the growing amount on a dial. I shall now show for the first time an instrument that I have devised, that will do all this in the simplest possible manner. I have here two wheels connected by a driving band of indiarubber, round which I have tied every few inches a piece of white silk ribbon. I shall turn one a little way, and hold the other. The driving force is indicated by a difference of stretching, the pieces of silk are much further apart on the tight side than they are on the loose. I shall

Fig. 6.



now turn the handle, and cause the wheels to revolve; the motion of the band is visible to all. The indiarubber is travelling faster on the tight side than on the loose side, nearly twice as fast; this must be so, for as there is less material on the tight side than on the loose, there would be a gradual accumulation of the indiarubber round the driven pulley, if they travelled at the same speed; since there is no accumulation, the tight side must travel the fastest. Now it may be shown mathematically that the difference in the speeds is proportional both to the actual speed and to the driving strain; it is therefore a measure

of the power or work being transmitted, and the difference in the distance travelled is a measure of the work done. I have here a working machine which shows directly on a dial the amount of work done; this I will show in action directly. Instead of indiarubber, elastic steel is used. Since the driving pulley has the velocity of the tight side, and the driven of the loose side of the belt, the difference in the number of their turns, if they are of equal size, will measure the work. This difference I measure by differential gearing which actuates a hand on a dial. I may turn the handle as fast as I please; the index does not move, for no work is being done. I may hold the wheel, and produce a great driving strain; again the index remains at rest, for no work is being done. I now turn the handle quickly, and lightly touch the driven wheel with my finger. The resistance, small though it is, has to be overcome; a minute amount of work is being done, the index creeps round gently. I will now put more pressure on my finger, more work is being done, the index is moving faster; whether I increase the speed or the resistance the index turns faster; its rate of motion measures the power, and the distance it has moved, or the number of turns, measures the work done. That this is so I will show by an experiment. I will wind up in front of a scale a 7 lb. weight; the hand has turned one-third round. I will now wind a 28 lb. weight up the same height; the hand has turned four-thirds of a turn. There are other points of a practical nature with regard to this invention which I cannot now describe.

There is one other class of instruments which I have developed of which time will let me say very little. The object of this class of instruments is to divide the speed with which two registrations are being effected, and continuously record the quotient. In the instrument on the table two iron cones are caused to rotate in time with the registrations; a magnetized steel reel hangs on below. This reel turns about, and runs up or down the cones until it finds a place at which it can roll at ease. Its position at once indicates the ratio of the speeds which will be efficiency, horse-power per hour, or one thing in terms of another. Just as the integrators are derived from the steering of an ordinary bicycle, so this instrument is derived from the double steering of the "Otto" bicycle.

Though I am afraid that I have not succeeded in the short time at my disposal in making clear all the points on which I have touched, yet I hope that I have done something to remove the very prevalent opinion that meters for power and electricity do not exist.

[C. V. B.]

GENERAL MONTHLY MEETING,

Monday, March 5, 1883.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

The Earl of Dalhousie, K.T.
James D. Bradshaw, Esq. M.A.
F. Werneck T. de Castro, Esq.
Mrs. Elizabeth Dobson,
Bryan Donkin, Esq. Jun.
Miss Clara Gisborne,
William Gonne, Esq.
Major William Hanmer,
Walter Harris, Esq.
Sir Charles Brodie Locock, Bart.
George Henry Pinckard, Esq.
Charles Richardson, Esq.

were elected Members of the Royal Institution.

The following arrangements for the Lectures after Easter were announced:—

Professor John G. McKendrick, M.D. F.R.S.E. Fullerian Professor of Physiology, R.I.—Ten Lectures on Physiological Discovery: A Retrospect, Historical, Biographical, and Critical; On Tuesdays, April 3, 10, 17, 24; Monday, April 30; Tuesdays, May 8, 15, 22, 29, and June 5.

Dr. Waldstein, Hon. M.A. Cantab.—Four Lectures on the Art of Pheidias; on Thursdays, April 5, 12, 19, 26.

Professor Tyndall, D.C.L. F.R.S.—Three Lectures on Count Rumford, Originator of the Royal Institution; on Thursdays, May 3, 10, 17.

Reginald Stuart Poole, Esq.—Three Lectures on Recent Discoveries in (1) Egypt, (2) Chaldæa and Assyria, (3) Cyprus and Asia Minor; on Thursdays, May 24, 31, and June 7.

Archibald Geikie, Esq. LL.D. F.R.S.—Six Lectures on Geographical Evolution; on Saturdays, April 7, 14, 21, 28, and May 5, 12.

Professor C. E. Turner, of the University of St. Petersburg.—Four Lectures. Historical Sketches of Russian Social Life; on Saturdays, May 19, 26, and June 2, 9.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Accademia dei Lincei, Reale, Roma—Atti, Serie Terza. Vol. VII. Fasc. 3. 4to. 1882.
Mémorie della Classe di Scienze Morali, Storiche e Filologiche. Vols. VII. IX. 4to. 1880–1.
Mémorie della Classe di Scienze Fisiche, Matematiche e Naturali. Vols. IX. X. 4to. 1880–1.

Antiquaries, Society of—Proceedings, Vol. VIII. No. 6. 8vo. 1882.
Asiatic Society of Bengal—Proceedings, No. 9. 8vo. 1882.
Astronomical Society, Royal—Monthly Notices, Vol. XLIII. No. 3. 8vo. 1883
Bankers, Institute of—Journal, Vol. IV. Part 2. 8vo. 1883.
British Architects, Royal Institute of—Proceedings, 1882–3, Nos. 8, 9. 4to.
Chemical Society—Journal for Feb. 1883. 8vo.
Index to Journal, Vols. XLI. and XLII. 8vo. 1882.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Series II. Vol. III. Part 1. 8vo. 1883.
Dialectical Society, London—Quarterly Journal of Transactions, No. 4. 8vo. 1883.
Domville, William Henry, Esq. M.R.I.—Reports of Hungarian Natural History Museum, Part V. Nos. 1–4. 8vo. Buda Pest, 1881–2.
East India Association—Journal, Vol. XIV. No. 5. 8vo. 1882.
Editors—American Journal of Science for Feb. 1883. 8vo.
Analyst for Feb. 1883. 8vo.
Athenæum for Feb. 1883. 4to.
Chemical News for Feb. 1883. 4to.
Engineer for Feb. 1883. fol.
Horological Journal for Feb. 1883. 8vo.
Iron for Feb. 1883. 4to.
Nature for Feb. 1883. 4to.
Revue Scientifique and Revue Politique et Littéraire for Feb. 1883. 4to.
Telegraphic Journal for Feb. 1883. fol.
Franklin Institute—Journal, No. 686. 8vo. 1882.
Gas Institute—Rules and List of Members. 8vo. June, 1882.
Geographical Society, Royal—Proceedings, New Series, Vol. V. No. 2. 8vo. 1883.
Geological Society—Quarterly Journal, No. 153. 8vo. 1883.
Abstracts of Proceedings, 1882–3, Nos. 431–433. 8vo.
Greenhill, A. G. Esq. M.A. (the Author)—On the Motion of a Projectile in a Resisting Medium. 8vo. 1882.
Johns Hopkins University—American Chemical Journal, Vol. IV. No. 5. 8vo. 1883.
Lisbon, Sociedade de Geographia—Bulletin, 3e Serie, No. 6. 8vo. 1882.
Droits du Portugal : Memorandum. 8vo. 1883.
Manchester Geological Society—Transactions, Vol. XVII. Parts 3, 4. 8vo. 1882–3.
Medical and Chirurgical Society—Proceedings, New Series, No. 1. 8vo. 1882.
Catalogue of Library, Supplement II. Additions 1881–2. 8vo. 1883.
Norwegian North-Atlantic Expedition—Editorial Committee :
H. Friele—Mollusca. 4to. Christiania, 1882.
L. Schmelck—Chemistry. 4to. Christiania, 1882.
Numismatic Society—Numismatic Chronicle and Journal. 3rd Series. No. 8. 8vo. 1882.
Pharmaceutical Society of Great Britain—Journal, Feb. 1883. 8vo.
Calendar, 1883. 8vo.
Ramsay, A.—Scientific Roll, No. 10. 8vo. 1883.
Royal Dublin Society—Transactions, Vol. I. Nos. 15–19; Vol. II. No. 2. 4to. 1882.
Proceedings, Vol. III. Part 5. 8vo. 1882.
St. Bartholomew's Hospital—Reports, Vol. XVIII. 8vo. 1882.
Smith, E. Noble, Esq. (the Author)—Curvatures of the Spine. 8vo. 1883.
Society of Arts—Journal, Feb. 1883. 8vo.
Symons, G. J. Esq. F.R.S. (the Compiler, &c.)—Monthly Meteorological Magazine, Feb. 1883. 8vo.
Telegraph Engineers, Society of—Journal, Vol. XI. No. 45. 8vo. 1883.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1883: Heft 1. 4to.
Winn, J. M. M.D. M.R.C.P. (the Author)—Darwin. 8vo. 1883.

WEEKLY EVENING MEETING,

Friday, March 9, 1883.

Sir Frederick Pollock, Bart. M.A. Vice-President, in the Chair.

Professor George D. Liveing, M.A. F.R.S.

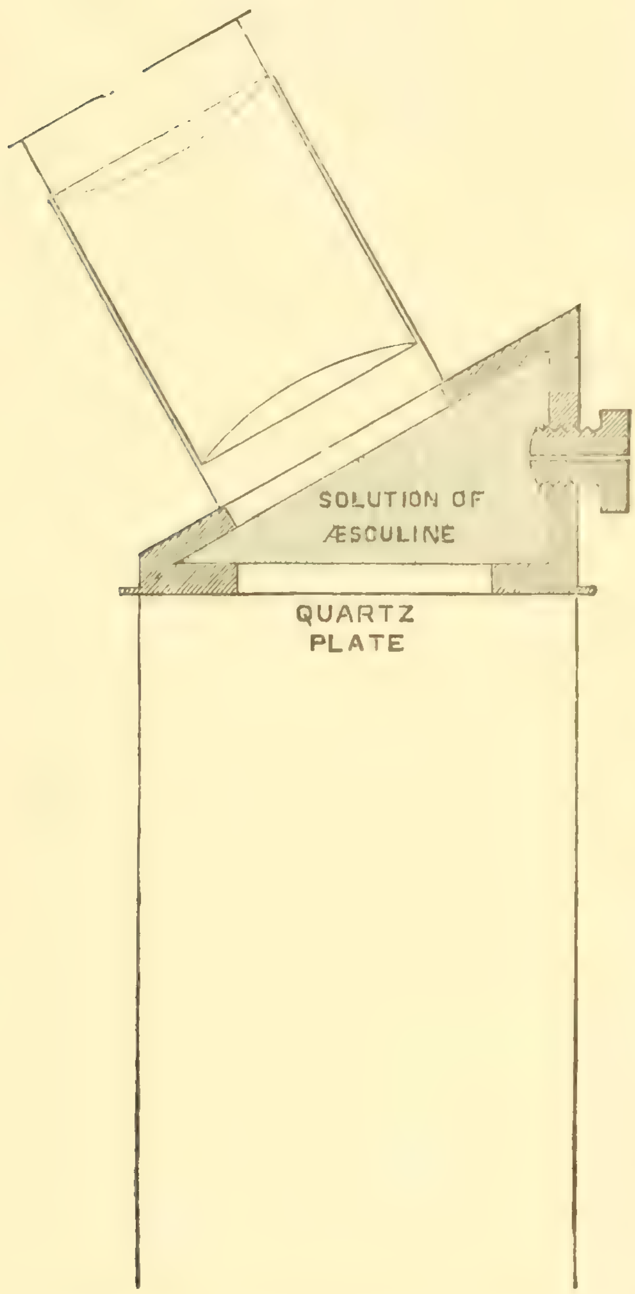
The Ultra-Violet Spectra of the Elements.

[t seems probable that the range of our vision as regards colour is :losely connected with the intensity of that part of the solar radiation which reaches us on the earth, for Langley's observations on the in- :ensity of the sun's rays in different parts of the spectrum bring out :he fact that the region of greatest intensity falls nearly in the middle of the visible spectrum, and includes those colours to which our eyes ıre most sensitive. The ultra-violet rays, those which lie beyond the violet on the more refrangible side, are not, however, absolutely nvisible, for, by carefully excluding light of lower refrangibility, Herschel found that he could see some distance beyond the Fraun- hofer line H, into what he called the lavender-grey; and Helmholtz ıas succeeded in seeing nearly all the strong lines in the solar spec- rum almost or quite up to its limit. Still these rays may fairly be said to be beyond ordinary vision; and from their power of chemical ıction they used to be distinguished as "actinic" rays. We know ıow that they have no monopoly of chemical activity, and we recognise ıo difference between luminous and actinic rays, the visible and the ıltra-violet, except in their oscillation frequencies; that is, in the ate at which the successive pulsations of the ray succeed one another, ınd in the colour and refrangibility which are directly dependent on hat rate. That the ultra-violet part of the solar spectrum extended ıt least as far above the line H as F is below it, has been known ince the time of Wollaston, who observed its effect in blackening ilver salts; but it is only about twenty years since Stokes made :nown to us the great length and intensity of the ultra-violet pectrum of the electric spark. Stokes used his own invention, a luorescent screen, for observing the rays; and at the very time vhen Stokes published his discovery, W. A. Miller published photo- graphs of the spectra of sparks taken between various metallic electrodes. Both these methods, that of fluorescent screens and that of photography, have been used by Professor Dewar and me in our esearches. For the method of fluorescence we have used a modifica- ion of Soret's eye-piece, substituting for the uranium glass-plate a vedge-shaped vessel full of a solution of æsculine, placed with ts edge horizontal so that we look down on the fluorescent liquid. The wedge form of the vessel has the advantage of refracting out of he line of vision all the rays except those which produce fluorescence,

a matter of no small importance when faint light is to be observed (see Fig. 1).

Now, although the intensity of the sun's rays falls away rapidly beyond the Fraunhofer line H, and comes to nothing about as far above H as F is below it, it is far otherwise with the radiation of our terrestrial elements when heated up in the electric spark or arc, or even in some cases in flames; some of those elements which we know to be abundant in the sun, such as iron and magnesium, exhibit their most intense radiation, their strongest and most persistent rays, in the ultra-violet region, in waves which succeed one another at the shortest intervals. Indeed those metals so readily take up certain ultra-violet vibrations, that when there is much metal in the arc, and it is confined in a crucible of lime or magnesia, they often give their characteristic lines strongly reversed, dark absorption-bands being produced by the slightly cooled vapour which is outside the arc. This is seen in the photographic plate Nos. 1 to 3. No. 2 shows the strongest magnesium line, in a region beyond the limit of the solar spectrum, at wave length 2852, expanded and reversed. No. 1 shows it enormously expanded, its bright wings reversing iron lines up to S. No. 3 shows a strong group of iron lines, still more refrangible, also expanded and reversed by putting iron wire into the arc. The dark bands in the photograph are due to absorption by the metallic vapour, and in their places strong bright lines appear when less metal is present. The spectrum of iron is of all metals the most complicated, and those of the other elements which are most closely related to iron in chemical characters come next to it in the number and complication of their ultra-violet lines. Manganese and chromium are especially remarkable for showing many groups of closely-set lines. No. 2 shows a group of chromium lines between the solar lines S and U. It is probably not without significance that this group of elements

Fig. 1.



which exhibit the greatest variety in their chemical relations, and produce combinations of the greatest number of types, and the most complicated spectra, are also those which produce the most highly-coloured compounds. In marked contrast to the thick-set ranks of iron, manganese, and chromium lines, are the few scattered rays exhibited by those metals which form their combinations each chiefly on a single type, such as aluminium, and the alkali and alkaline earth metals. These spectra are probably even simpler than at first sight they seem to be. That of lithium is the simplest (Plate II. fig. 3): a series of single lines succeeding one another at decreasing intervals, and with diminishing intensity, closely resembling in these respects the spectrum of hydrogen. In the case of hydrogen, we know that the oscillation frequencies of some of its rays are related in a simple harmonic ratio. We are not able to say that the relation is so simple in the case of lithium; but still the whole series are probably overtones of a fundamental vibration, not so simply related as the harmonics of a uniform stretched string, but, like the overtones of a string which is not of uniform thickness, or is loaded at different points, similarly related in origin, though not exact harmonics. That the different rays are in many cases so related as overtones of a fundamental vibration appears more plainly, perhaps, when not single lines but groups of two, three, or four lines recur. Potassium shows a series of pairs to which the well-known violet pair, and perhaps that in the red also, belong. Calcium, magnesium, and zinc, each show a series of triplets, which are alternately sharply defined and diffuse (see photographs 4 and 5 and Plate II. figs. 5 and 6). In other cases the same characters may be traced, though less readily, because there is sometimes more than one such series of lines or groups. The alkali metals have each one such series in the visible spectrum, and another in the ultra-violet. It may happen in other cases that two or more such series overlap, and it may then be very difficult to distinguish and separate them.

In some cases elements show at a lower temperature a far more complicated spectrum than they do at higher temperatures further removed from their points of liquefaction. This has been observed by Roscoe and Schuster in the case of the alkali metals potassium and sodium, which give at temperatures only a little above their boiling-points absorption spectra which consist of closely-set fine lines, producing an appearance of shaded bands quite unlike their emission spectra at higher temperatures. In some few cases we have observed similar "fluted" or "venetian blind" spectra, as they have been called, in the ultra-violet, as, for example, one produced by tin; but in general the temperature of the arc, which we have chiefly used in our observations on metals, is high enough to carry the metals beyond the stage in which their vibrations are constrained by the state approaching to liquefaction.

But though metals do not often show spectra of this class at the high temperature of the arc, it is otherwise with metalloids and

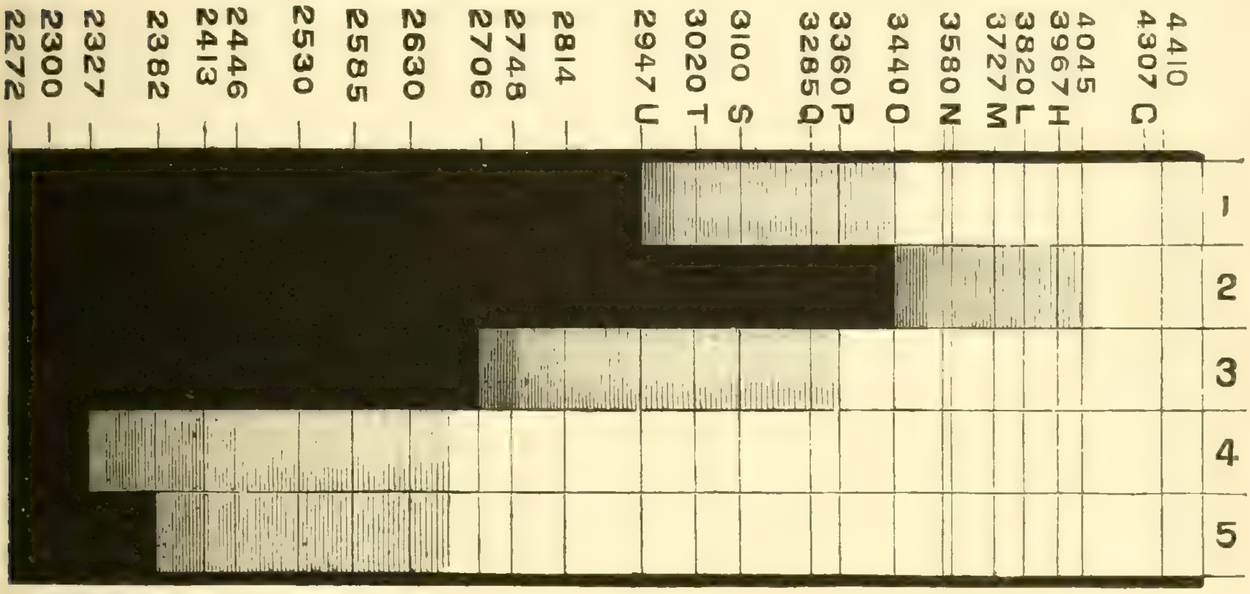
with compounds. Nitrogen gives in the arc as well as in the spark a channelled spectrum of singular beauty, extending with but short breaks almost to the extremity of the ultra-violet region which we have examined. (See photograph 7.) These multitudinous lines of nitrogen constantly present in the arc taken in air, help to make the problem of unravelling the spectrum of the arc, and assigning each line to its proper source, far more difficult than it might at first sight be supposed. Carbon, which in the arc frequently gives a channelled spectrum in the visible region, gives only a limited number of lines in the ultra-violet; but cyanogen gives one set of flutings near the line L, and another near N, which are so brilliant in the arc as to obscure the metallic lines in their neighbourhood (photograph 4). To the same class we may refer the spectrum of water, of which the most brilliant portion is given in photograph 6.

The series of lines produced by the same element, which I have spoken of as overtones of a fundamental vibration, have been likened to these channellings, but in reality they are very different. In the series, which I have supposed to have a sort of harmonic relation, the successive lines or groups of lines invariably become nearer to one another as the wave-lengths become shorter, and at the same time they diminish in strength and sharpness; whereas in the channelled spectra the strongest lines are at the end where they are most closely set, and they generally diminish in strength as they get further apart. Also increase of distance between the lines of channelled spectra is sometimes towards the less, sometimes towards the more, refrangible end of the spectrum.

I have before observed that a great part of the ultra-violet spectra of the elements which we have observed lies entirely beyond the limit of the solar spectrum; that limit is the line U at wave-length 2947. But though this is the limit of the solar radiation which reaches us on the earth, we can hardly suppose that the sun itself, or the photosphere, emits no radiation of shorter wave-length. We know that there is plenty of iron and magnesium in the sun, and the strongest radiations at high temperatures of these elements are of shorter wave-length than U. Moreover, the continuous spectra of incandescent solids in many cases extend far beyond U. The continuous spectrum of burning magnesium reaches quite up to the wave-length 2380, that of the flame of carbon disulphide mixed with hydrogen and fed with oxygen reaches even further, that of lime heated with an oxyhydrogen blowpipe, though feeble beyond the limit of the solar spectrum, extends up to wave-length 2680. The temperature of the sun cannot be less than that of any of these sources of heat, so that we are forced to suppose that the radiation, more refrangible than U, which leaves the body of the sun, is stopped somewhere either in our atmosphere, or in planetary space, or in the atmosphere of the sun himself. Now Cornu has found that when the thickness of our atmosphere traversed by the sun's rays is diminished as much as possible by taking the sun at its greatest

altitude, and making the observation from an elevated station (the Riffelberg), the solar spectrum only reaches to wave-length 2932, that is, only a very trifle beyond U. We must therefore suppose that the absorbent substance, whatever it be, is not in our atmosphere. The same reason will lead us to reject the notion that the absorption can be due to matter in planetary space, for it is not easy to suppose that the gases which pervade that space in extreme tenuity can differ much from those in our atmosphere, because the earth in its annual

FIG. 2.



No. 1. Solar spectrum.
No. 2. Candle flame.
No. 3. Lime light.
No. 4. Carbon disulphide and hydrogen flame.
No. 5. Magnesium flame.

course must pick them up whatever they are, and they must then diffuse into our atmosphere, and we must in time have them in a more condensed state in our atmosphere than in planetary space. The absorbent is therefore probably neither in our atmosphere nor in planetary space, and we must look for it in the solar atmosphere. When we notice how much of the radiation of our terrestrial elements is of shorter wave-length than the solar line U, we might almost fancy that the blotting out of the sun's light beyond that point is simply due to an increase in the number and breadth of the Fraunhofer lines. Indeed we have frequently observed the strong magnesium line, wave-length 2852, expanded so that the dark absorption band in its middle reached quite up to U on one side (see photograph No. 1) and equally far on the other side, and this, together with such expansion of the strong iron lines beyond as we have occasionally observed, would go a long way towards completely hiding all light above U. But such expansions of iron and magnesium lines, high in the scale of refrangibility, do not occur without a considerable expansion of the lines of the same elements lower in the scale, expansions far exceeding what we actually observe in the Fraunhofer lines. Moreover the Fraunhofer lines, though

dark by comparison with the brightness of the photosphere, are themselves luminous, even bright, when there is no other still brighter light wherewith to contrast them, so that if there were no other absorbent action the solar spectrum would be continued by the emitted rays of the metallic vapours which produce these lines. Probably then the absorbent is something at a lower temperature, higher in the solar atmosphere. A change of temperature may, and in some cases certainly does imply such a change of state that there may be a corresponding change in the particular vibrations which can be most easily taken up.

The metals in the liquid and solid states are so very opaque that we should hardly be able to discern their absorption spectra; nevertheless in very thin films they are translucent in different degrees. Gold leaf, as is well-known, transmits a green light, and we have found that a thin film of gold chemically deposited on a plate of quartz is fairly transparent for all the ultra-violet rays, so that its selective absorption is almost wholly of the less refrangible rays. Silver deposited in a similar way produces a very different effect. It is almost wholly opaque, except for one rather narrow band which begins a little below the solar line P and extends with diminishing transparency to about S. Cornu has before noticed this property of silver, but placed the transparent band at wave-length 270 instead of 330. Dr. W. A. Miller had observed that the light reflected by gold is equally distributed all through the ultra-violet, but feebler than that reflected by other metals; while that reflected by silver is characterised by giving a sudden cessation of the photographic image for a certain distance. These characters of the reflected rays he attributed to absorption by the metal.

When we examine the absorption produced by the haloid elements, we find that chlorine absorbs a wide band in the ultra-violet with its centre near the solar line P, extending, when the chlorine is in small quantity, from N to T, increasing in width on both sides when the quantity of chlorine is increased, but still leaving the rays above wave-length 2550 unabsorbed.

Bromine vapour shows an absorption band which begins in the visible spectrum, and extends, when the bromine is in small quantity, up to L, and when the bromine is in greater quantity up to P. From that point, up to about wave-length 2500, the vapour is transparent, but beyond it is again absorbent, the absorption increasing gradually with the refrangibility of the rays.

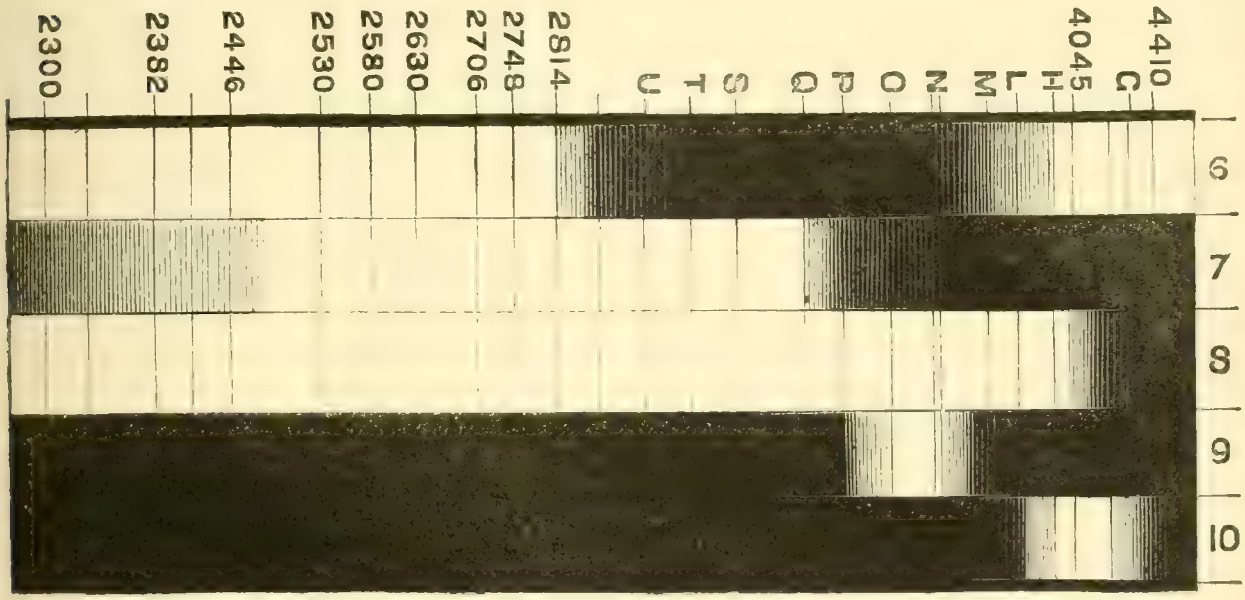
Iodine vapour, when thin, is transparent for ultra-violet rays, but produces strong absorption in the violet region. With thicker vapour this absorption extends nearly to H, but the vapour is still transparent for rays more refrangible than H.

Lecoq de Boisbaudran has observed that in the spectra of similar elements we may trace a shifting of similar lines, or groups of lines, towards the less refrangible side as the atomic weight is increased. Thus the violet pair of lines given by potassium is represented by an indigo pair in the case of rubidium, and a blue pair in the case of

cæsium; and the indigo line of calcium is represented by a blue line in the spectrum of strontium, and by a green line in the spectrum of barium.

We may observe something of the same kind in regard to the haloid elements: the absorption band which in the case of the element of lowest atomic weight, namely chlorine, is altogether ultra-violet, is shifted towards the less refrangible side in the case of bromine, and lies altogether in the visible region in the case of iodine, the element of highest atomic weight.

FIG. 3.



No. 6. Absorption of chlorine.
No. 7. ,, ,, bromine vapour.
No. 8. ,, ,, iodine vapour.
No. 9. ,, ,, bromine liquid.
No. 10. ,, ,, iodine solution.

It is remarkable that bromine in the liquid state and iodine in solution show absorptions quite different from those of their vapours. A thin film of liquid bromine between two quartz plates is transparent for a band which ends just where the transparency of the vapour begins, while the film is opaque for rays both above and below this band. Iodine dissolved in carbon disulphide is also transparent for a certain distance, but the band is shifted to a less refrangible region lying between G and H.

Compound gases and vapours show, as might be expected, various absorptions of ultra-violet rays. The absorbent action of coal-gas begins at about the wave-length 2680, and above 2580 it is nearly complete. Sulphurous acid has an absorption band extending from about R (3179) to rays of wave-length 2630, with a weaker absorption extending some way beyond these limits on both sides. Sulphuretted hydrogen produces a pretty complete obliteration of all rays above wave-length 2580. Vapour of carbon disulphide in very small quantity produces an absorption extending from P to T, shading away at each end. With more vapour this band widens, and a second absorption band begins at about the wave-length 2580. Chlorine peroxide gives

a succession of nine shaded bands at nearly equal intervals between M and S, while in the highest regions of the spectrum it seems to be quite transparent.

I mentioned at the outset the probable connection between the intensity of the solar radiation and the sensitiveness of our eyes to rays of different colours. The consideration of ultra-violet absorption spectra leads to the mention of another fact connected with vision, or rather with the construction of the eyes of the higher animals. Soret has investigated, and recently Chardonnet has more fully examined, the limits of transparency of the crystalline, cornea, and vitreous humour of the eyes of various animals and man, and found them all more or less transparent for ultra-violet rays. The limit of transparency in many cases approaches, but never exceeds, the limit of the solar spectrum. Chardonnet places the limit of transparency of the crystalline of the human eye as low as M, which is not consistent with the observations of Herschel and Helmholtz before mentioned, but this inconsistency is probably due to alterations which had taken place after death in the eyes experimented on by Chardonnet. That the transparency of the materials of the eye does not extend beyond the solar line U, Chardonnet regards as a provision of nature to protect the retina from the extreme radiations of artificial lights; but I venture to offer a different explanation, which is, that the selection of the materials of the eye has been determined not by what they will absorb but by what they will transmit. If the materials in question were in any great degree opaque to the ultra-violet solar rays, these rays must be absorbed and must either be used in heating the absorbent or do work upon it in some form, perhaps alter it chemically, and so impair its efficiency as part of an optical instrument. I see, then, in the selection of these materials for our eyes an instance, one amongst many, of the marvellous adaptation of our organisation to the natural, rather than to the artificial surroundings in which we are placed. [G. D. L.]

DESCRIPTION OF THE PLATES.

In the photographic plate:—

No. 1 shows an expansion of the magnesium line at wave-length 2852, while it is also so strongly reversed as to produce a complete obliteration of all the lines above U within the range of the photograph, while its bright wing reverses the iron lines near T.

No. 2 shows the same line at *b* much less expanded but still self-reversing. The lines at *a* are also magnesium lines, wave-lengths 2795 and 2801. Most of the lines between *b* and *S* are chromium lines.

No. 3 shows iron lines reversed by putting iron wire into the arc.

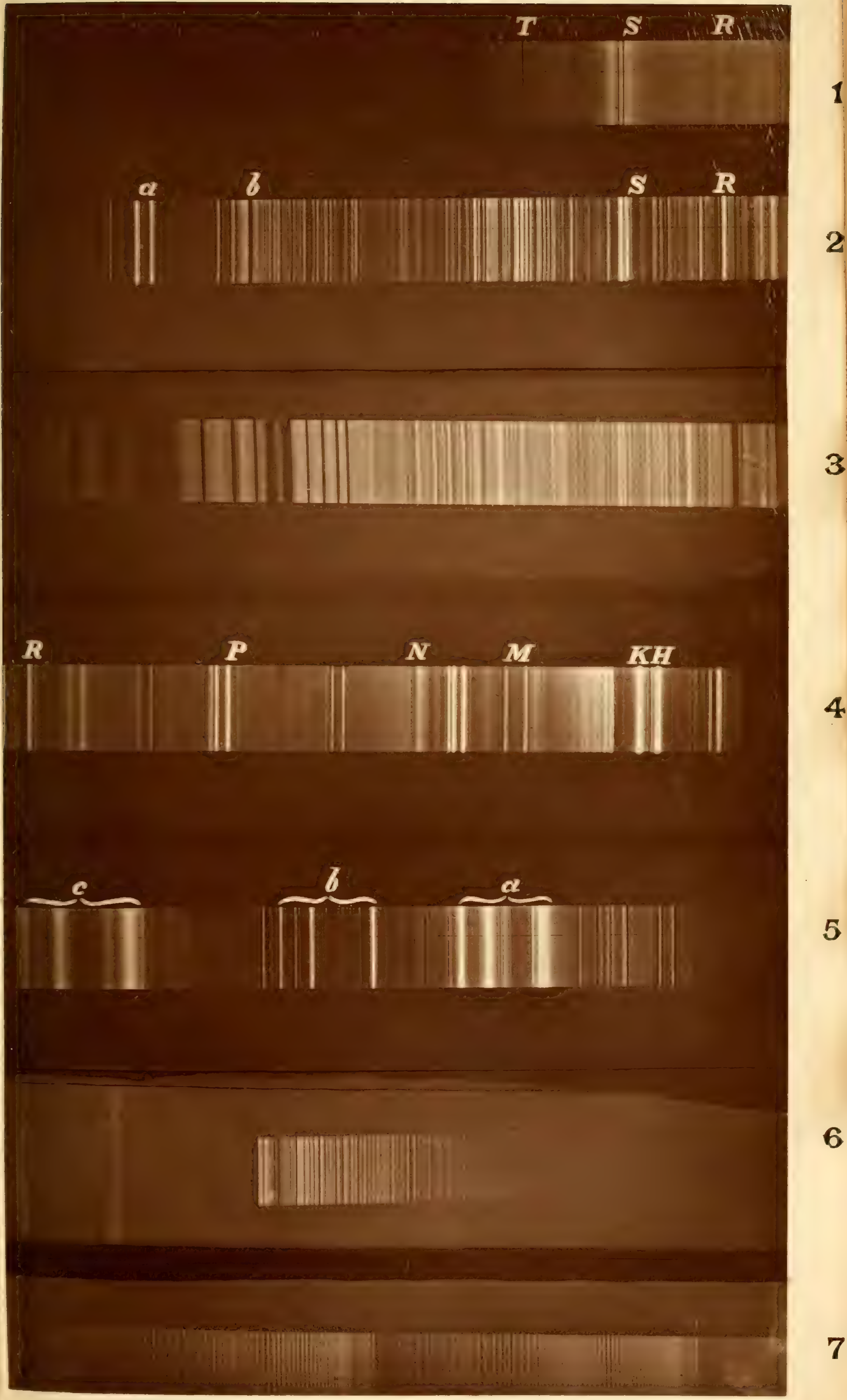
No. 4 shows calcium lines in recurring triplets; also the cyanogen bands between K and M and at N.

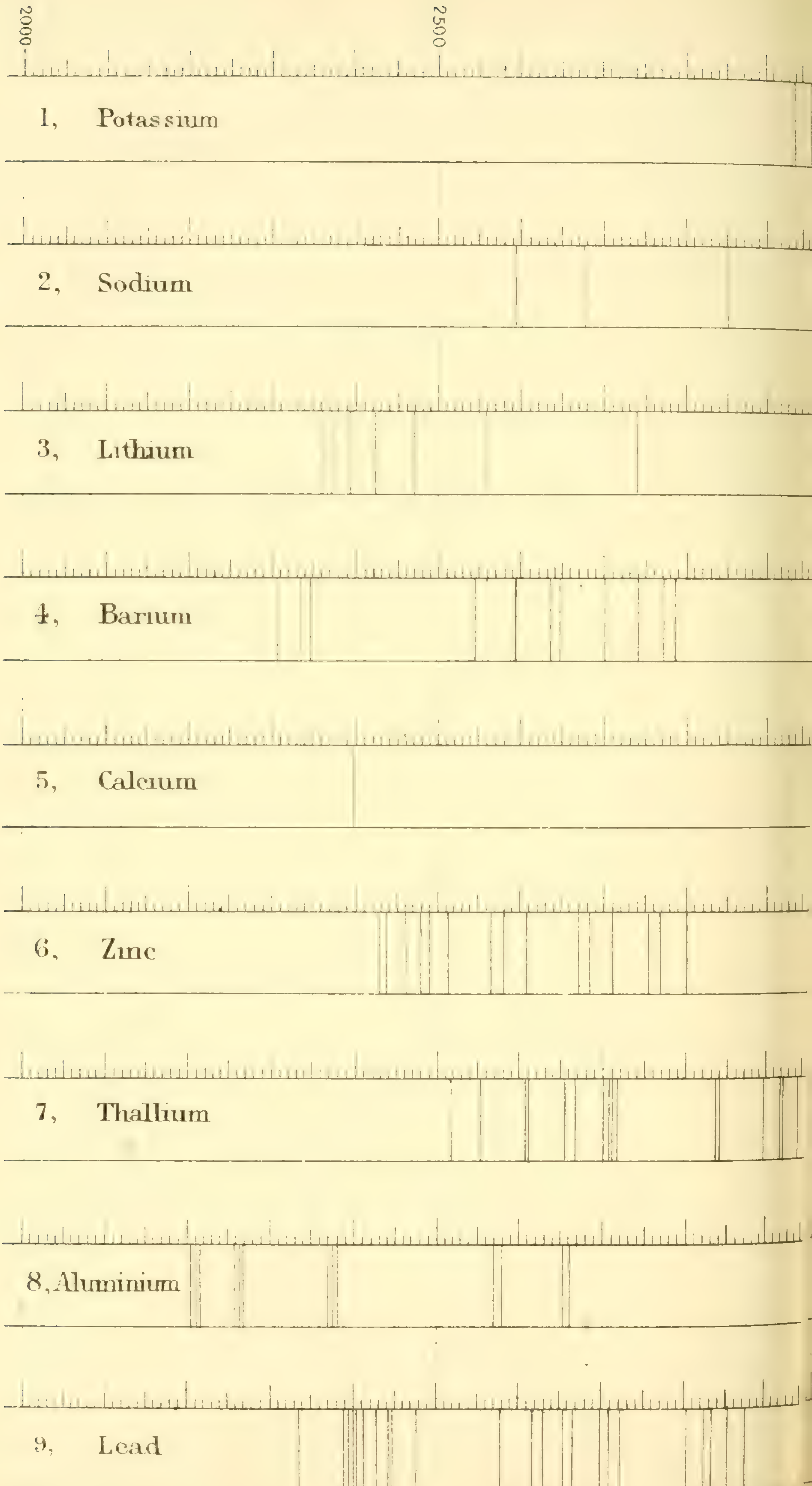
No. 5 shows three of the zinc triplets.

No. 6 is the brightest part of the ultra violet spectrum of water.

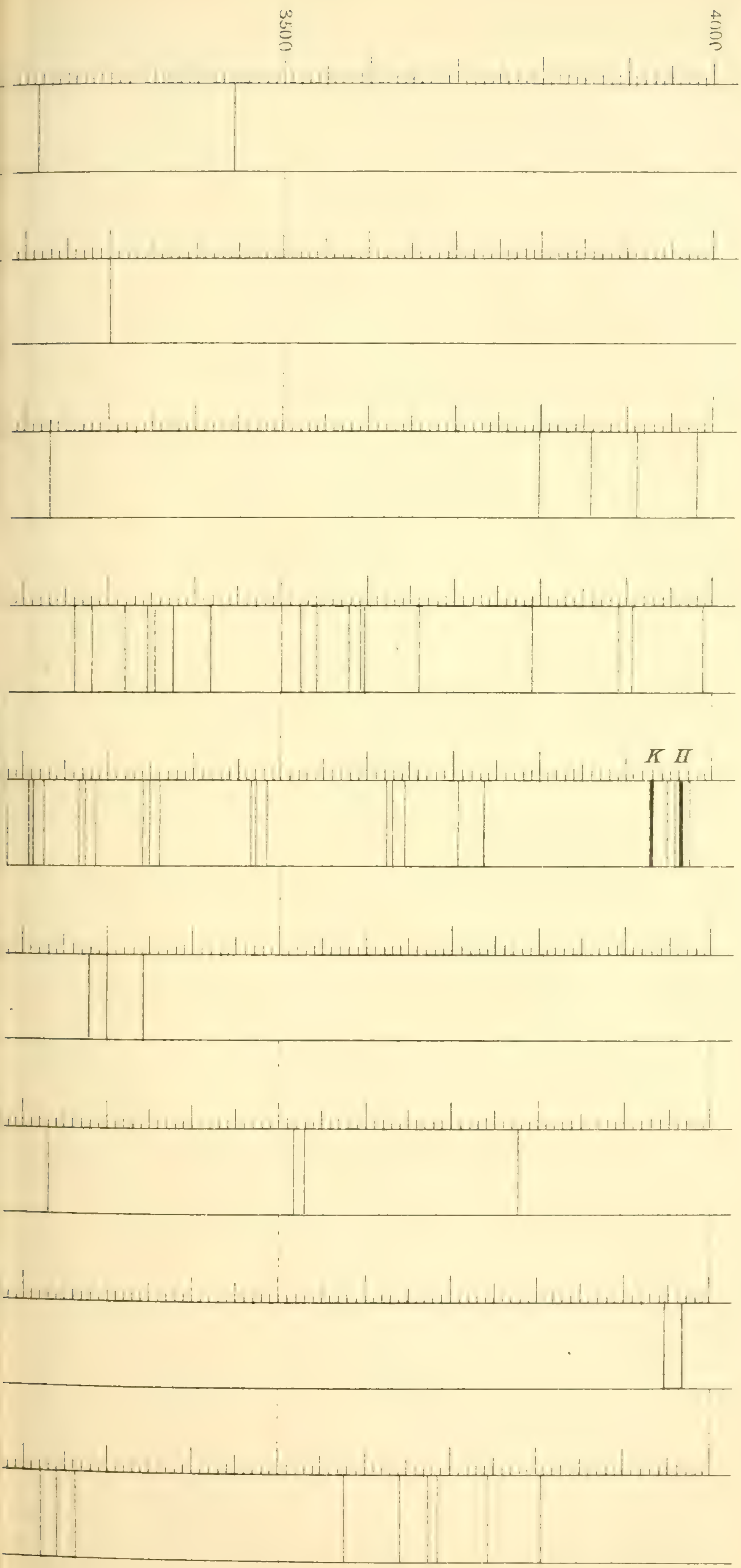
No. 7 shows part of the channelled spectrum of nitrogen in the ultra violet.

The lithographic plate gives the position of the ultra violet lines of several metals to a scale of wave lengths.


T
S
R
1
a
b
S
R
2
3
R
P
N
M
KH
4
c
b
a
5
6
7



2000
2500
1, Potassium
2, Sodium
3, Lithium
4, Barium
5, Calcium
6, Zinc
7, Thallium
8, Aluminium
9, Lead


3500
4000
K H

WEEKLY EVENING MEETING,

Friday, March 16, 1883.

William Bowman, Esq. LL.D. F.R.S. Honorary Secretary and Vice-President, in the Chair.

Professor Tyndall, D.C.L. F.R.S. *M.R.I.*

Thoughts on Radiation, Theoretical and Practical.

Scientific discoveries are not distributed uniformly in time. They appear rather in periodic groups. Thus, in the two first years of this century, among other gifts presented by men of science to the world, we have the Voltaic pile; the principle of Interference, which is the basis of the undulatory theory of light; and the discovery by William Herschel of the dark rays of the sun.

Directly or indirectly, this latter discovery heralded a period of active research on the subject of radiation. Leslie's celebrated work on the Nature of Heat was published in 1804, but he informs us, in the preface, that the leading facts which gave rise to the publication presented themselves in the spring of 1801. An interesting but not uncommon psychological experience is glanced at in this preface. The inconvenience of what we call ecstacy, or exaltation, is that it is usually attended by undesirable compensations. Its action resembles that of a tidal river, sometimes advancing and filling the shores of life, but afterwards retreating and leaving unlovely banks behind. Leslie, when he began his work, describes himself as "transported at the prospect of a new world emerging to view." But further on the note changes, and before the preface ends he warns the reader that he may expect variety of tone, and perhaps defect of unity in his disquisition. The execution of the work, he says, proceeded with extreme tardiness; and as the charm of novelty wore off, he began to look upon his production with a coolness not usual in authors.

The ebb of the tide, however, was but transient; and to Leslie's ardour, industry, and experimental skill, we are indebted for a large body of knowledge in regard to the phenomena of radiation. In the prosecution of his researches he had to rely upon himself. He devised his own apparatus, and applied it in his own way. To produce radiating surfaces, he employed metallic cubes, which to the present hour are known as Leslie's cubes. The different faces of these cubes he coated with different substances, and filling the cubes with boiling

water, he determined the emissive powers of the substances thus heated. These he found to differ greatly from each other. Thus, the radiation from a coating of lampblack being called 100, that from the uncoated metallic surface of his cube was only 12. He pointed out the reciprocity existing between radiation and absorption, proving that those substances which emit heat copiously absorb it greedily. His thermoscopic instrument was the well-known differential-thermometer invented by himself. In experiment Leslie was very strong, but in theory he was not so strong. His notions as to the nature of the agent whose phenomena he investigated with so much ability are confused and incorrect. Indeed, he could hardly have formed any clear notion of the physical meaning of radiation before the undulatory theory of light, which was then on its trial, had been established.

A figure still more remarkable than Leslie occupied the scientific stage at the same time, namely, the vigorous, original, and practical Benjamin Thompson, better known as Count Rumford, the originator of the Royal Institution. Rumford traversed a great portion of the ground occupied by Leslie, and obtained many of his results. As regards priority of publication, he was obviously discontented with the course which things had taken, and he endeavoured to place both himself and Leslie in what he supposed to be their right relation to the subject of radiant heat. The two investigators were unknown to each other personally, and their differences never rose to scientific strife. There can hardly, I think, be a doubt that each of them worked independently of the other, and that where their labours overlap, the honour of discovery belongs equally to both.

The results of Leslie and Rumford were obtained in the laboratory; but the walls of a laboratory do not constitute the boundary of its results. Nature's hand specimens are always fair samples, and if the experiments of the laboratory be only true, they will be ratified throughout the universe. The results of Leslie and Rumford were in due time carried from the cabinet of the experimenter to the open sky by Dr. Wells, a practising London physician. And here let it be gratefully acknowledged that vast services to physics have been rendered by physicians. The penetration of Wells is signalised among other things by the fact recorded by the late Mr. Darwin, that forty-five years before the publication of the 'Origin of Species,' the London doctor had distinctly recognised the principle of Natural Selection, and that he was the first to recognise it. But Wells is principally known to us through his 'Theory of Dew,' which, prompted by the experiments of Leslie and Rumford, and worked out by the most refined and conclusive observations on the part of Wells himself, first revealed the cause of this beautiful phenomenon. Wells knew that through the body of our atmosphere invisible aqueous vapour is everywhere diffused. He proved that grasses and other bodies on which dew was deposited were powerful emitters of radiant heat; that when nothing existed in the air to stop

their radiation, they became self-chilled; and that while thus chilled they condensed into dew the aqueous vapour of the air around them. I do not suppose that any theory of importance ever escaped the ordeal of assault on its first enunciation. The theory of Wells was thus assailed; but it has proved immovable, and will continue so to the end of time.

The interaction of scientific workers causes the growth of science to resemble that of an organism. From Faraday's tiny magneto-electric spark, shown in this theatre half a century ago, has sprung the enormous practical development of electricity at the present time. Thomas Seebeck in 1822 discovered thermo-electricity, and eight years subsequently bars of bismuth and antimony were first soldered together by Nobili so as to form a thermo-electric pile. In the self-same year Melloni perfected the instrument and proved its applicability to the investigation of radiant heat. The instrumental appliances of science have been well described as extensions of the senses of man. Thus the invention of the thermopile vastly augmented our powers over the phenomena of radiation. Melloni added immensely to our knowledge of the transmission of radiant heat through liquids and solids. His results appeared at first so novel and unexpected that they excited scepticism. He waited long in vain for a favourable Report from the Academicians of Paris; and finally, in despair of obtaining it, he published his results in the Annales de Chimie. Here they came to the knowledge of Faraday, who, struck by their originality, brought them under the notice of the Royal Society, and obtained for Melloni the Rumford medal. The medal was accompanied by a sum of money from the Rumford fund; and this, at the time, was of the utmost importance to the young political exile, reduced as he was to penury in Paris. From that time until his death, Melloni was ranked as the foremost investigator in the domain of radiant heat.

As regards the philosophy of the thermopile, and its relation to the great doctrine of the conservation of energy, now everywhere accepted, a step of singular significance was taken by Peltier in 1834. Up to that time it had been taken for granted that the action of an electric current upon a conductor through which it passed, was always to generate heat. Peltier, however, proved that, under certain circumstances, the electric current generated cold. He soldered together a bar of antimony and a bar of bismuth, end to end, thus forming of the two metals one continuous bar. Sending a current through this bar, he found that when it passed from antimony to bismuth across the junction, heat was always there developed, whereas when the direction of the current was from bismuth to antimony, there was a development of cold. By placing a drop of chilled water upon the junction of the two metals, Lenz subsequently congealed the water to ice by the passage of the current.

The source of power in the thermopile is here revealed, and a relation of the utmost importance is established between heat and

electricity. Heat is shown to be the nutriment of the electric current. When one face of a thermopile is warmed, the current produced, which is always from bismuth to antimony, is simply heat consumed and transmuted into electricity.

Long before the death of Melloni, what the Germans call "Die Identitäts Frage," that is to say, the question of the identity of light and radiant heat, agitated men's minds and spurred their inquiries. In the world of science people differ from each other in wisdom and penetration, and a new theoretic truth has always at first the minority on its side. But time, holding incessantly up to the gaze of inquirers the unalterable pattern of nature, gradually stamps that pattern on the human mind. For twenty years Henry Brougham was able to quench the light of Thomas Young, and to retard, in like proportion, the diffusion of correct notions regarding the nature and propagation of radiant heat. But such opposing forces are, in the end, driven in, and the undulatory theory of light being once established, soon made room for the undulatory theory of radiant heat. It was shown by degrees that every purely physical effect manifested by light was equally manifested by the invisible form of radiation. Reflection, refraction, double refraction, polarization, magnetization, were all proved true of radiant heat, just as certainly as they had been proved true of light. It was at length clearly realised that radiant heat, like light, was propagated in waves through that wondrous luminiferous medium which fills all space, the only real difference between them being a difference in the length and frequency of the ethereal waves. Light, as a sensation, was seen to be produced by a particular kind of radiant heat, which possessed the power of exciting the retina.

And now we approach a deeper and more subtle portion of our subject. What, we have to ask, is the origin of the ether waves, some of which constitute light, and all of which constitute radiant heat? The answer to this question is that the waves have their origin in the vibrations of the ultimate particles of bodies. But we must be more strict in our definition of ultimate particles. The ultimate particle of water, for example, is a *molecule*. If you go beyond this molecule and decompose it, the result is no longer water, but the discrete *atoms* of oxygen and hydrogen. The molecule of water consists of three such atoms tightly held together, but still capable of individual vibration. The question now arises: Is it the molecules vibrating as wholes, or the shivering atoms of the molecules that are to be considered as the real sources of the ether waves? As long as we were confined to the experiments of Leslie, Rumford, and Melloni, it was difficult to answer this question. But when it was discovered that gases and vapours possessed—in some cases to an astonishing extent—the power both of absorbing and radiating heat, a new light was thrown upon the question.

You know that the theory of gases and vapours, now generally accepted, is that they consist of molecular or atomic projectiles darting to and fro, clashing and recoiling, endowed, in short, with a motion not of vibration but of translation. When two molecules clash, or when a single molecule strikes against its boundary, the first effect is to deform the molecule, by moving its atoms out of their places. But gifted as they are with enormous resiliency, the atoms immediately recover their positions, and continue to quiver in consequence of the shock. Held tightly by the force of affinity, they resemble a string stretched to almost infinite tension, and therefore capable of generating tremors of almost infinite rapidity. What we call the heat of a gas is made up of these two motions—the flight of the molecules through space, and the quivering of their constituent atoms. Thus does the eye of science pierce to what Newton called "the more secret and noble works of Nature," and make us at home amid the mysteries of a world lying in all probability vastly further beyond the range of the microscope than the range of the microscope, at its maximum, lies beyond that of the unaided eye.

The great principle of radiation, which affirms that all bodies absorb the same rays that they emit, is now a familiar one. When, for example, a beam of white light is sent through a yellow sodium flame, produced by a copious supply of sodium vapour, the yellow constituent of the white beam is stopped by the yellow flame, and if the beam be subsequently analysed by a prism, a black band is found in the place of the intercepted yellow band of the spectrum. We have been led, as you know, to our present theoretic knowledge of light by a close study of the phenomena of sound, which in the present instance will help us to a conception of the action of the sodium flame. The atoms of sodium vapour synchronize in their vibrations with the particular waves of ether which produce the sensation of yellow light. The vapour, therefore, can take up or absorb the motion of those waves, as a stretched piano-string takes up or absorbs the pulses of a voice pitched to the note of the string. I will now show you the action of sodium vapour, in a way and with a result which startled and perplexed me on first making the experiment, more than twenty years ago. You know that the spectra of incandescent metallic vapours are not continuous, but formed of brilliant bands. I wished, in 1861, to obtain the brilliant yellow band produced by incandescent sodium vapour. To this end, I placed a bit of sodium in a carbon crucible, and volatilized it by a powerful voltaic current. A feeble spectrum overspread the screen, from which I thought the sodium band would stand out with dominant brilliancy. To my surprise, at the very point where I expected this brilliant band to appear, a band of darkness took its place. By humouring the voltaic arc a little, the darkness vanished, and in the end I obtained the bright band which I had sought at the beginning. On reflection the cause was manifest

The first ignition of the sodium was accompanied by the development of a large amount of sodium vapour, which spread outwards and surrounded, as a cool envelope, the core of intensely heated vapour inside. By the cool vapour the rays from the hot were intercepted, but on lengthening the arc the outer vapour in great part was dispersed, and the rays passed to the screen. This relation as to temperature was necessary to the production of the black band; for were the outside vapour as hot as the inside, it would, by its own radiation, make good the light absorbed.

An extremely beautiful experiment of this kind was made here last week by Professor Liveing, with rays which, under ordinary circumstances, are entirely invisible. Professor Dewar and Professor Liveing have been long working with conspicuous success at the ultra-violet spectrum, and with Professor Dewar's aid I will now show you this spectrum, as it was shown last week by Professor Liveing. Using prisms and lenses of a certain kind, and a powerful dynamo machine to volatilize our metals, we cast a spectrum upon the screen. You notice the terminal violet of this spectrum. Far beyond that violet, waves are now impinging upon the screen, which have no sensible effect upon the organ of vision; they constitute what we call the ultra-violet spectrum. Professor Stokes has taught us how to render this invisible spectrum visible, and it is by a skilful application of Stokes' discovery that Liveing and Dewar bring the hidden spectrum out with wondrous strength and beauty.

You notice here a small second screen, which can be moved into the ultra-violet region. Felt by the hand, the surface of this screen resembles sandpaper, being covered with powdered uranium glass, a highly fluorescent body. Pushing the moveable screen towards the visible spectrum, at a distance of three or four feet beyond the violet, light begins to appear. On pushing in the screen, the whole ultra-violet spectrum falls upon it, and is rendered visible from beginning to end. The spectrum is not continuous, but composed for the most part of luminous bands derived from the white-hot crucible in which the metals are to be converted into vapour. I beg of you to direct your attention on one of these bands in particular. Here it is, of fair luminous intensity. My object now is to show you the reversal, as it is called, of that band which belongs to the vapour of magnesium, exactly as I showed you a moment ago the reversal of the sodium band. An assistant will throw a bit of magnesium into the crucible, and you are to observe what first takes place. The action is rapid, so that you will have to fix your eyes upon this particular strip of light. On throwing in the magnesium, the luminous band belonging to its vapour is cut away, and you have, for a second or so, a dark band in its place. I repeat the experiment three or four times in succession, with the same unfailing result. Here, as in the case of the sodium, the magnesium surrounded itself for a moment by a cool envelope of its own

vapour, which cut off the radiation from within, and thus produced the darkness.

And now let us pass on to an apparently different, but to a really similar result. Here is a feebly luminous flame, which you know to be that of hydrogen, the product of combustion being water vapour. Here is another flame of a rich blue colour, which the chemists present know to be the flame of carbonic oxide, the product of combustion being carbonic acid. Let the hydrogen flame radiate through a column of ordinary carbonic acid—the gas proves highly transparent to the radiation. Send the rays from the carbonic oxide flame through the same column of carbonic acid—the gas proves powerfully opaque. Why is this? Simply because the radiant, in the case of the carbonic oxide flame, is hot carbonic acid, the rays from which are quenched by the cold acid exactly as the rays from the intensely heated sodium vapour were quenched a moment ago by the cooler envelope which surrounded it. Bear in mind the case is always one of synchronism. It is because the atoms of the cold acid vibrate with the same frequency as the atoms of the hot, that the pulses sent forth from the latter are absorbed.

Newton, though probably not with our present precision, had formed a conception similar to that of molecules and their constituent atoms. The former he called corpuscles, which, as Sir John Herschel says, he regarded "as divisible groups of atoms of yet more delicate kind." The molecules he thought might be seen if microscopes could be caused to magnify three or four thousand times. But with regard to the atoms, he made the remark already alluded to:—"It seems impossible to see the more secret and nobler works of nature within the corpuscles, by reason of their transparency."

I have now to ask your attention to an illustration intended to show how radiant heat may be made to play to the mind's eye the part of the microscope, in revealing to us something of the more secret and noble works of atomic nature. Chemists are ever on the alert to notice analogies and resemblances in the atomic structures of different bodies. They long ago pointed out that a resemblance exists between that evil-smelling liquid, bisulphide of carbon, and carbonic acid. In the latter substance, we have one atom of carbon united to two of oxygen, while in the former we have one atom of carbon united to two of sulphur. Attempts have been made to push the analogy still further by the discovery of a compound of carbon and sulphur which should be analogous to carbonic oxide, where the proportions, instead of one to two, are one to one, but hitherto, I believe, without success. Let us now see whether a little physical light cannot reveal an analogy between carbonic acid and bisulphide of carbon more occult than any hitherto pointed out. For all ordinary sources of radiant heat the bisulphide, both in the liquid and vaporous form, is the most transparent, or diathermanous, of bodies. It transmits, for example, 90 per cent. of the radiation from our hydrogen flame, 10 per cent.

only being absorbed. But when we make the carbonic oxide flame our source of rays, the bisulphide shows itself to be a body of extreme opacity. The transmissive power falls from 90 to about 25 per cent., 75 per cent. of the radiation being absorbed. To the radiation from the carbonic oxide flame the bisulphide behaves like the carbonic acid. In other words, the group of atoms constituting the molecule of the bisulphide vibrate in the same periods as those of the atoms which constitute the molecule of the carbonic acid. And thus we have established a new, subtle, but most certain resemblance between these two substances. The time may come when chemists will make more use than they have hitherto done of radiant heat as an explorer of molecular condition.

The term " theoretical radiation " introduced into the title of this discourse is, I hope, thus justified. The conception of these quivering atoms is a theoretic conception, but it is one which gives us a powerful grasp of the facts, and enables us to realise mentally the mechanism on which radiation and absorption depend. We will turn in a moment to what I have called practical " radiation." It is pretty well known that for a long series of years I conducted an amicable controversy with one of the most eminent experimenters of our time, as regards the action of the earth's atmosphere on solar and terrestrial radiation. My contention was that the great body of our atmosphere—its oxygen and nitrogen—had but little effect upon either the rays of the sun coming to us, or the rays of the earth darting away from us into space, but that mixed with the body of our air there was an attenuated and apparently trivial constituent which exercised a most momentous influence. That body, as many of you know, is aqueous vapour, the amount of which does not exceed 1 per cent. of the whole atmosphere. Minute, however, as its quantity is, the life of our planet depends upon that vapour. Without it, in the first place, the clouds could drop no fatness. In this sense the necessity for its presence is obvious to all. But it acts in another sense as a preserver. Without it as a covering, the earth would soon be reduced to the frigidity of death. Observers were, and are, slow to take in this fact, which nevertheless is a fact, however improbable it may at first sight appear. The action of aqueous vapour upon radiant heat has been established by irrefragable experiments in the laboratory; and these experiments, though not unopposed, have been substantiated by some of the most accomplished meteorologists of our day.

I wished much to instruct myself a little by actual observation on this subject, under the open sky, and my first object was, to catch, if possible, states of the weather which would enable me to bring my views to a practical test. Thanks to an individual who devotes her life to taking care of mine, a little iron hut, embracing a single room, has been placed for my benefit, upon the wild moorland of Hind Head. From the plateau on which the hut stands, there is a free outlook in all directions. Here, amid the heather, I had two

stout poles fixed firmly in the ground eight feet asunder, and a stout cord stretched from one to the other. From the centre of this cord a thermometer is suspended with its bulb four feet above the ground. On the ground is placed a pad of cotton wool, and on this cotton wool a second thermometer, the object of the arrangement being to determine the difference of temperature between the two thermometers, which are only four feet vertically apart.

Permit me at the outset to deal with the subject in a perfectly elementary way. In comparison with the cold of space, the earth must be regarded as a hot body, sending its rays, should nothing intercept them, across the atmosphere into space. The cotton wool is chosen because it is a powerful, though not the most powerful, radiator. It pours its heat freely into the atmosphere, and by reason of its flocculence, which renders it a non-conductor, it is unable to derive from the earth heat which might atone for its loss. Imagine the cotton wool thus self-chilled. The air in immediate contact with it shares its chill, and the thermometer lying upon it partakes of the refrigeration. In calm weather the chilled air, because of its greater density, remains close to the earth's surface, and in this way we sometimes obtain upon that surface a temperature considerably lower than that of the air a few feet above it. The experiments of Wilson, Six, and Wells have made us familiar with this result. On the other hand, the earth's surface during the day receives from the sun more heat than it loses by its own radiation, so that when the sun is active, the temperature of the surface exceeds that of the air.

These points will be best illustrated by describing the course of temperature for a day, beginning at sunrise and ending at 10.20 P.M. on March 4. The observations are recorded in the annexed table, at the head of which is named the place of observation, its elevation above the sea, and the state of the weather. The first column in the Table contains the times at which the two thermometers were read. The column under "Air" gives the temperatures of the air, the column under "Wool" gives the temperatures of the wool, while the fourth column gives the differences between the two temperatures. It is seen at a glance that from sunrise to 9.20 A.M. the cotton wool is colder than the air; at 9.30 the temperatures are alike. This is the hour of "intersection," which is immediately followed by "inversion." Throughout the day and up to 4 P.M. the wool is warmer than the air. At 4.5 P.M. the temperatures are again alike; while from that point downwards the loss by terrestrial radiation is in excess of the gain derived from all other sources, the refrigeration reaching a maximum at 7.30 P.M., when the difference between the two thermometers amounted to 10° Fahr. When the observations are continued throughout the night, the greater cold of the surface is found to be maintained until sunrise, and for some hours beyond it. Had the air been perfectly still during the observations, the nocturnal chilling of

the surface would have been in this case greater; for you can readily understand that even a light wind sweeping over the surface, and mixing the chilled with the warmer air, must seriously interfere with the refrigeration.

Hind Head, Elevation, 850 feet.

Course of Temperature, March 4th, 1883.

Sky cloudless. Hoar frost. Wind light from north-east.

Time.	Air.	Wool.	Difference.
6.50 A.M. (sunrise)	31°	25°	6°
7.20	32½	24½	8
7.40	34	25	9
8.5	35	27	8
8.20	35	30	5
9.15	40	38	2
9.20	41	40	1
9.30 (intersection)	41	41	0
9.40 (inversion)	41	42	1
10.15	42½	45	2½
11.	45	52	7
11.30	47	55	8
12. noon	50	58	8
12.30 P.M.	50	59½	9½
1.	50	57½	7½
2.	49	60	11
2.30	48	58	10
3.	49	56	7
3.30	48	52	4
4.	47	48	1
4.5 (intersection)	47	47	0
4.10 (inversion)	47	45	2
4.15	47	43	4
4.30	46	41	5
7.	35	26	9
7.30	35	25	10
8·30	34	24½	9½
9.40	33	24½	8½
10·20	32	24	8

Glacial wind from north-east. Stars very bright.

Various circumstances may contribute to lessen, or even abolish, the difference between the two thermometers. Haze, fog, cloud, rain, snow, are all known to be influential. These are visible impediments to the outflow of heat from the earth; but my position for some time has been that a very powerful obstacle to that outflow exists which is entirely invisible. The pure vapour of water, for example, is a gas as invisible as the air itself. It is everywhere diffused through the air; but, unlike the oxygen and nitrogen of the atmosphere, it is not constant in quantity. We have now to examine whether meteoro-

logical observations do not clearly indicate its influence on terrestrial radiation.

With a view to this examination, I will choose a series of observations made during the afternoon and evening of a day of extraordinary calmness and serenity. The visible condition of the atmosphere at the time was that which has hitherto been considered most favourable to the outflow of terrestrial heat, and therefore best calculated to establish a large difference between the air and wool thermometers. The 16th of last January was a day of this kind, when the observations recorded in the annexed table were made.

January 16th.—Extremely serene. Air almost a dead calm. Sky without a cloud. Light south-westerly air.

Time.	Air.	Wool.	Difference.
P.M.	°	°	°
3.40	43	37	6
3.50	42	35	7
4.	41	35	6
4.15	40	34	6
4.30	38	32	6
5.	37	28	9
5.30	37	30	7
6.	36	32	4
6.30	36	31	5
7.	36	28	8
7.30	35½	28	7½
8.	35	26	9
8.30	34	25	9
9.	35	27	8
10.	35	28	7
10.30	35	29	6

During these observations there was no visible impediment to terrestrial radiation. The sky was extremely pure, the moon was shining; Orion, the Pleiades, Charles's Wain, including the small companion star at the bend of the shaft, the North Star, and numbers of others, were clearly visible. After the last observation, my notebook contains the remark, "Atmosphere exquisitely clear; from zenith to horizon cloudless all round."

A moment's attention bestowed on the column of differences in the foregoing table will repay us. Why should the difference at 6 P.M. be fully 5° less than at 5 P.M.; and again 5° less than at 8 and at 8.30 respectively? There was absolutely nothing in the aspect of the atmosphere to account for the approach of the two thermometers at 6 o'clock—nothing to account for their preceding and subsequent divergence from each other. Anomalies of this kind have been observed by the hundred, but they have never been accounted for, and they did not admit of explanation until it had been proved that the

intrusion of a perfectly invisible vapour was competent to check the radiation, while its passing away re-opened a doorway into space.

It is well to bear in mind that the difference between the two thermometers on the evening here referred to varied from 4° to 9°, the latter being the maximum.

Such observations might be multiplied, but, with a view to saving space, I will limit the record. On the evening of January 30th, the atmosphere was very serene; there was no moon, but the firmament was powdered with stars. At 7.15 P.M. the difference between the two thermometers was 6°; while at 9.30 P.M. it was 4°, the wool thermometer being in both cases the colder of the two. On February 3rd observations were made under similar conditions of weather, and with a similar result. At 7.15 P.M. the difference between the thermometers was 6°; while at 8.25 P.M. it was 4°. On both these evenings the sky was cloudless, the stars were bright, while the movement of the air was light, from the south-west.

In all these cases the air passing over the plateau of Hind Head had previously grazed the comparatively warm surface of the Atlantic Ocean, where it had charged itself with aqueous vapour to a degree corresponding to its temperature. Let us contrast its action with that of air coming to Hind Head from a quarter less competent to charge it with aqueous vapour. We were visited by such air on the 10th of last December, when the movement of the wind was light from the north-east, the temperature at the time, moreover, was very low, and hence calculated to lessen the quantity of atmospheric vapour. Snow a foot deep covered the heather. At 8.5 A.M. the two thermometers were taken from the hut, having a common temperature of 35°. The one was rapidly suspended in the air, and the other laid upon the wool. I was not prepared for the result. A single minute's exposure sufficed to establish a difference of 5° between the thermometers; an exposure of five minutes produced a difference of 13°; while after ten minutes' exposure the difference was found to be no less than 17°. Here follow some of the observations.

December 10th.—Deep snow; low temperature; sky clear; light north-easterly air.

Time.	Air.	Wool.	Difference.
A.M.	°	°	°
8.10	29	16	13
8.15	29	12	17
8.20	27	12	15
8.30	26	11	15
8.40	26	10	16
8.45	27	11	16
8.50	29	11	18

During these observations, a dense bank of cloud on the opposite ridge of Blackdown, virtually retarded the rising of the sun. It had,

however, cleared the bank during the last two observations, and, touching the air thermometer with its warmth, raised its temperature from 26° to 27° and 29°. The very large difference of 18° is in part to be ascribed to this raising of the temperature of the air thermometer. I will limit myself to citing one other case of a similar kind. On the evening of the 31st of March, though the surface temperature was far below the dew point, very little dew was deposited. The air was obviously a dry air. The sky was perfectly cloudless, while the barely perceptible movement of the air was from the north-east. At 10 P.M. the temperature of the air thermometer was 37°, that of the wool thermometer was 20°, a refrigeration of 17° being therefore observed on this occasion.

From the behaviour of a smooth ball when urged in succession over short grass, over a gravel walk, over a boarded floor, and over ice, it has been inferred that, were friction entirely withdrawn, we should have no retardation. In a similar way, under atmospheric conditions visibly the same, we observe that the refrigeration of the earth's surface at night markedly increases with the dryness of the atmosphere: we may infer what would occur if the invisible atmospheric vapour were entirely withdrawn. I am far from saying that the body of the atmosphere exerts no action whatever upon the waves of terrestrial heat; but only that its action is so small that, when due precautions are taken to have the air pure and dry, laboratory experiments fail to reveal any action. Without its vaporous screen, our solid earth would practically be in the presence of stellar space; and with that space, so long as a difference existed between them, the earth would continue to exchange temperatures. The final result of such a process may be surmised. If carried far enough, it would infallibly extinguish the life of our planet.

[J. T.]

GENERAL MONTHLY MEETING,

Monday, April 2, 1883.

GEORGE BUSK, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Arthur Goulston, Esq. Jun. B.A. Cantab.
R. Raymond Mège, Esq.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Mr. L. M. Rate, the Secretary of the Board of Visitors, for his donation of £20 towards the purchase of the new Gas Engine.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Accademia dei Lincei, Reale, Roma—Atti, Serie Terza. Vol. VII. Fasc. 4. 4to. 1883.
Asiatic Society of Bengal—Journal, Vol. LI. Part 2, No. 4. 8vo. 1883.
Astronomical Society, Royal—Monthly Notices, Vol. XLIII. No. 4. 8vo. 1883.
Bankers, Institute of—Journal, Vol. IV. Part 3. 8vo. 1883.
British Architects, Royal Institute of—Proceedings, 1882–3, Nos. 10, 11. 4to.
British Museum Trustees—Catalogue of the Batrachia Salientia. 2nd Ed. 8vo. 1882.
Catalogue of the Batrachia Gradientia and Apoda. 2nd Ed. 8vo. 1882.
Catalogue of the Fossil Foraminifera. 8vo. 1882.
List of Hymenoptera. Vol. I. Tenthredinidæ and Siricidæ. 8vo. 1882.
Catalogue of Oriental Coins. Vol. VII. 8vo. 1882.
Guides to the Exhibition Galleries. 8vo. 1881–3.
Guides to Index Museum, and to the Collections of Meteorites and Gould's Birds. 8vo. 1882–3.
Index to Collection of Minerals. 8vo. 1882.
Chemical Society—Journal for March, 1883. 8vo.
Editors—American Journal of Science for March, 1883. 8vo.
Analyst for March, 1883. 8vo.
Athenæum for March, 1883. 4to.
Chemical News for March, 1883. 4to.
Engineer for March, 1883. fol.
Horological Journal for March, 1883. 8vo.
Iron for March, 1883. 4to.
Nature for March, 1883. 4to.
Revue Scientifique and Revue Politique et Littéraire for March, 1883. 4to.
Telegraphic Journal for March, 1883. fol.
Franklin Institute—Journal, No. 687. 8vo. 1883.
Geographical Society, Royal—Proceedings, New Series, Vol. V. No. 3. 8vo. 1883.

Geological Society—Abstracts of Proceedings, 1882–3, Nos. 434, 435. 8vo.
Guest, Mrs. E.—Origines Celticae, and other Contributions to the History of Britain. By E. Guest. 2 vols. 8vo. 1883.
Johns Hopkins University—American Journal of Philology, No. 12. 8vo. 1882.
University Circulars, No. 21. 4to. 1883.
Linnean Society—Journal, Nos. 96, 125. 8vo. 1883.
Proceedings, March, 1883. 8vo.
Lisbon, Sociedade de Geographia—Bulletin, 3e Serie, No. 7. 8vo. 1882.
Mactie, R. A. Esq. F.R.S.E. (the Author)—The Patent Bills for 1883. 8vo. 1883.
Manchester Geological Society—Transactions, Vol. XVII. Part 5. 8vo. 1882–3.
Meteorological Office—Hourly Readings, 1881. Part III. 4to. 1883.
Newton, A. V. Esq. (the Author)—The Patent Agent and his Profession. 8vo. 1883.
Pharmaceutical Society of Great Britain—Journal, March, 1883. 8vo.
Photographic Society—Journal, New Series, Vol. VII. No. 5. 8vo. 1883.
Polydore, E. E. Esq. (the Inventor)—Explication du Calendrier Perpétuel. 8vo. Smyrna, 1882.
Purdy, Frederick, Esq. F.S.S. M.R.I.—Annual Report of the Local Government Board. Supplement. Report of Medical Officer, 1881. 8vo. 1882.
Rio de Janeiro, Observatoire Impérial de—Annales, Tome I. 4to. 1882.
Royal Society of London—Proceedings, No. 223. 8vo. 1882.
Society of Arts—Journal, March, 1883. 8vo.
St. Petersbourg Académie des Sciences—Mémoires, 7e Serie, Tome XXX. Nos. 9–11. 4to. 1882.
Symons, G. J. Esq. F.R.S.—Monthly Meteorological Magazine, March, 1883. 8vo.
Telegraph Engineers, Society of—Journal, Vol. XI. No. 46. 8vo. 1883.
Vaux, W. S. W. Esq. M.A. F.R.S. M.R.I.—The Russian Railway to Herat and India. By C. Marvin. 8vo. 1883.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1883: Heft 2. 4to.
Vernon-Harcourt, L. F. Esq. M.A. (the Author)—The Floods around Oxford. 8vo. 1883.
Wild, Dr. H. (the Director)—Annalen des Physikalischen Central Observatoriums, 1881. Theil II. 4to. 1882.

WEEKLY EVENING MEETING,

Friday, April 6, 1883.

William Bowman, Esq. LL.D. F.R.S. Honorary Secretary and Vice-President, in the Chair.

Archibald Geikie, Esq. F.R.S.
Director-General of the Geological Survey of the United Kingdom.

The Cañons of the Far West.

The drainage lines of a country are among the most permanent features in its topography. Where a river has once fastened its grasp upon the surface, there it for the most part remains, and nothing short of some colossal upheaval can turn it into a new course. The rivers of Europe have served as types of fluviatile action in geology. Yet an examination of the conditions under which they work shows that they have been impeded by influences of various kinds. Except in their mountain tributaries, they flow over comparatively low land. Their gentle declivity prevents them from attaining any great erosive power, and, as one result of this characteristic, they have cut comparatively few deep narrow winding gorges. The geological structure of this continent is moreover so complicated, that hard and soft rocks are thrown together in rapid alternation, and little scope is afforded for the excavation of continuous ravines. The climate, too, being comparatively moist, much general disintegration of the surface takes place, and the detritus washed off by rain loads the rivers nearly to the maximum of their transporting power. Where, therefore, as is usually the case, the rivers flow in open valleys with gently sloping sides, the rate of atmospheric denudation has been at least as great as, or greater than, that of river erosion. Where, on the other hand, they flow in narrow precipitous ravines, they have been able to erode faster than the atmospheric agents have worn down the surrounding surface. The influence of vegetation has likewise affected the general disintegration of the surface. But perhaps the most important factor has been the glaciation of the Ice-Age. A large part of the area was under ice at that period. The minor pre-glacial contours were then in great measure obliterated, either by being ground down by the movement of the ice-sheets, or by being buried under the masses of clay, earth, and stones spread out over the lower grounds and valleys on the retreat of the ice. The river-channels would especially suffer. The valleys that existed before the advent of the ice generally remained valleys after the ice had gone, but the actual channels of the post-glacial drainage would only occasionally

coincide with those of older date. Hence the present river-channels in the glaciated regions of Europe cannot have a higher antiquity than the later part of the Glacial period. The European rivers therefore do not offer illustrations of river action in its most active phase.

Probably the region of the earth's surface, where the erosive action of rivers can be witnessed on the grandest scale is the basin of the Colorado river between southern Wyoming and the desert plateaux of Arizona. Throughout that region the strata are generally so slightly inclined that to the eye they appear to be horizontal. They form vast plains and elevated plateaux, the higher portions being densely clothed with pine-forests, while the lower tracts are in large measure barren desert. The strangest feature in this strange region is the way in which the surface has been trenched by running water into a system of profound gorges or cañons. Through the main cañons flows the rushing turbid Colorado river, but save where its few tributaries join, the side cañons have no permanent streams, and are only occasionally the beds of torrents when a heavy rain-shower falls. Nearly the whole of the water of the Colorado comes from distant high grounds, and the cañon region through which the river winds is in great part dry and desert. The Grand Cañon of the Colorado, according to the recent measurements of Captain Dutton, is 220 miles long, and in places about 6000 feet deep. There are really two cañons—a wide outer valley five or six miles broad, with precipitous sides descending 2000 or 3000 feet to a platform through which winds the inner gorge 3000 feet deep and from 3500 to 4000 feet from crest to crest. These colossal excavations are entirely the work of running water. No trace of any fracture has been found to account for them; on the contrary, they have been eroded across the line of large folds and dislocations of the rocks.

River-erosion is here at its maximum rate. This appears to depend partly on the high altitude of the region and the consequent great declivity and rapidity of the rivers, partly on the aridity of the climate, which permits the rivers to erode their beds unimpeded by the various causes that hinder them in Europe; and partly on the large amount of detritus washed down into the rivers by the occasional heavy rain-showers.

But there has likewise been simultaneously an enormous denudation of the surface of the plains and plateaux through which the Colorado flows. In the Grand Cañon district a mass of strata not less than 10,000 feet thick is computed to have been removed. From the broad fold of the Uinta Mountains the amount of rock swept away is estimated at $3\frac{1}{2}$ miles in vertical thickness. This vast denudation has been entirely the work of sub-aerial agents, and appears to have been in progress ever since the floor of the Cretaceous sea was gently raised into land in early Eocene times.

There may have been variations in the meteorological conditions of the past, so that the rate of denudation has possibly been

accelerated or diminished from time to time. At present, part of the disintegration is due to the superficial strain upon the surfaces of rock from the rapid expansion and contraction caused by the great daily range of temperature. The surface of the bare rocky wastes crumbles down. The wind brushes off the lighter particles, and makes use of them as a kind of sand-blast to wear down the rocks and stones over which they are driven. Rain, though infrequent, falls in occasional heavy showers, which, sweeping off the loosened materials, rush in torrents of mud and sand down the side cañons into the main gorge.

Much that is most characteristic in the scenery of the cañon region arises from the horizontality of the strata. The same beds of rock, with the same forms of weathering and the same peculiar colours, may be traced from cliff to cliff for many miles. A strangely regular and almost architectural symmetry thus arises, and is traceable even through the most rugged and broken parts of the scenery. The array of spires, towers, buttresses, alcoves, pediments, mouldings, cornices, along the escarpment of any stratum or group of strata is in continual decay and renewal. Slice after slice is cut away from the face of a cliff by the disintegration and removal of the softer beds underneath, and thus the escarpments are creeping backward across the plateaux. Old valleys now devoid of any living stream are no longer deepened, but the sapping of their boundary walls continues, so that they are becoming slowly wider. There are traces of an older system of drainage on the plateaux, but the channels are now dry and are even in some instances truncated by the walls of the main cañon. These deserted channels probably indicate a former period of greater moisture. The glacial period undoubtedly filled up the basin of the Great Salt Lake nearly a thousand feet above its present level, until, as a sheet of fresh water, it sent its drainage northwards into the Snake river, and thence into the Pacific Ocean. The author had searched in vain for evidence that the glaciers of the mountains to the west, north, and east of the cañon region had advanced into the plains, but they probably supplied a larger volume of water to the rivers of that region than these now possess.

The history of the erosion of the cañons, as shown by Powell and Dutton, throws light on the slow rate at which some of the grander movements of the earth's crust may take place. The Green river, as the northern part of the Colorado is called, instead of turning round the flank of the Uinta Mountains, strikes boldly into them, and has cut a series of deep cañons across the end of the chain. It can be shown that this operation cannot have been effected after the uplift but must have been contemporaneous with it. The upheaval of the vast fold of these mountains, from which some 24,000 feet of rock have been removed, must have been so slow as not to dislodge the river from the winding channel it had previously selected As fast as the mountains rose, the river sawed its way down through them. In the Grand Cañon region, faults of sometimes 6000 feet in displacement

have occurred across the pathway of the river, but without in any way deflecting it.

The influence of subterranean movements is shown in successive terraces with intervening platforms, as in other rivers. The terraces, however, do not consist of alluvium but of escarpments of solid rock.

In forecasting the future history of the cañons, we find that the Grand Cañon has somewhere about 1000 feet to remove from the bottom of its channel before its slope becomes so slight that its erosive power will nearly cease. It is conceivable that should no geological revolution occur in the region, the cañon may be still deepened to that amount. There are indications, however, that a limit may be set to the possible depth of the chasm. Like the *creep* of a coal mine, where the floor is squeezed upwards by the pressure of the enormous superincumbent weight on either side, the bottom of the cañon, relieved from the weight of the overlying column of rock, may be forced upwards by the pressure of the walls on either side. In that case, the channel might rise as fast as the river cut it down, so long as nothing occurred at the surface to materially diminish the height of the walls.

[A. G.]

WEEKLY EVENING MEETING,

Friday, April 13, 1883.

William Bowman, Esq. LL.D. F.R.S. Honorary Secretary and Vice-President, in the Chair.

Charles Waldstein, Esq. Ph.D. (Heidelberg), M.A. (Cambridge),

READER IN CLASSICAL ARCHÆOLOGY IN THE UNIVERSITY OF CAMBRIDGE.

*The Influence of Athletic Games upon Greek Art.**

If we were to tell an uneducated workman or a thoughtless young lady in society that the reasons why they consider certain people good-looking and certain things pleasing and well-proportioned are based upon the canons of proportion which the ancient Greeks established for the human form and for the objects manufactured by them, they would find it difficult to realise the truth of such a statement. Yet so it is. We can all readily realise that our taste differs from the taste of savages who, as Professor Flower told you three years ago, flatten their skulls, run bones through their noses, and stretch and thicken their lips and ears by means of weights. Yet the fundamental principles underlying our taste, in contradistinction to that of these savages, are identically the same as those laid down by the artists of ancient Hellas. Nay, the difference in taste which separates us from the types of Byzantine-Christian and early German art is the same as that which separates these pre-renaissance types from those of ancient Greece. We are fundamentally still Greek in taste. Now, when we consider how much further than the Byzantines and early Germans the ancient Greeks are removed from us in time, space, race, and religion, the question as to the causes of this singular persistency of the influence of Greek art must necessarily present itself to the inquiring mind.

The first and most obvious answer to this question is a purely historical one. It would consist in pointing to the events of the Italian renaissance, where, as a matter of fact, Greek art and Greek letters were brought back to light and revived among the people. Yet this is really no answer. For the question must further be asked: why should a few statues unearthed, coming from an age remote in antiquity, commend themselves so strongly to the taste of a later age

* This address is written from memory, as nearly as possible in the form in which it was delivered. In a few instances points that could only be mentioned in the address have been slightly amplified in writing. This is a preliminary publication, without illustrations and notes, of a work which the writer hopes some day to publish in a more complete form.

living under different conditions, as to supersede the prevailing canons? and why should such an "ancient innovation" not die as a passing fashion, but prevail and persist in its influence for centuries?

The reasons for this are not to be found in historical accident; but must be sought in the essential nature of Greek art itself, in those elements inherent in the art which give it the power of persisting and of retaining its validity for all ages and countries.

Two words, the weight of which I cannot hope to convey in this short address, contain in their combination the essential and distinctive characteristics of Greek art. It is the combination of Nature and the Ideal.

Greek art has persisted in its influence down to our time, because the artist clung to nature and followed her as a kind mother, and nature practically remains the same throughout all ages. Byzantine, not to speak of Egyptian and early Oriental, art have not done this. The types of Byzantine art are conventional abstractions from nature; a work of Greek art, however ideal, is instinct with nature.

But the persistent influence of Greek art is not wholly due to the fact that it was naturalistic; it is due above all to another quality of primary importance, namely, that it was ideal, that it idealised nature. The ideal in art is the highest generalisation of form. In Greek art it was the highest generalisation of the forms in nature. The works of Greek art are therefore not dependent for appreciation upon the individual spectator or one special mood of the individual, but are valid for all sane men, all men of a certain physiological constitution of their senses, surrounded by man and nature relatively the same. The works of the early Flemish and German masters, and even of some of the Kranachs and of Albert Dürer, are replete with nature, yet are often wanting in the ideal elements which raise the artist above the individual model into the realms of the most perfect ideal types.

Neither Nature alone nor the Ideal alone can make art lasting in its influence over different peoples and ages; it is only in the combination of the two, in nature idealised, that this persistent quality of art can be found. Yet Greek art was not always characterised by the combination of these two elements.

We naturally incline to forget the early elementary stages of things great and perfect. Nay, we are inclined to believe that they never were humble and lowly, never grew from the smaller and lower to the great and high. Thus the early childhood of Greek art is not widely known. Still, there was a period when Greek art was not possessed of nature. We may further generalise and say, that art never is possessed of nature in its earliest stages. It has often been stated that the origin of art is to be found in the imitative instinct of man. This experience shows to be utterly untrue. It is really to be found in man's active nature, in his creative instinct, which often drives him to seek for what is opposed to nature as she presents herself to him day by day, for rhyme as opposed to prose, for

harmony of tones as opposed to confusion of sound, for composition of thoughts, things and forms as opposed to haphazard collocation of things. Children, when set to draw a real house, will not imitate what they see before them, but will return to the well-known schematic form which we have all drawn, and which children in all countries have drawn and will draw. It is at a far later stage that man learns to render exactly what he sees before him and to follow and imitate nature.

The childhood of Greek art shows the same characteristics which mark the early artistic attempts of children. The works belonging to what is called the Archaic period (roughly speaking, the works previous to the fifth century before our era) all exhibit a conventional treatment which, when compared with the treatment of the works of the fifth and subsequent centuries, betokens a complete inability to render freely and accurately the forms of nature. How then was the step from this imperfection to the highest perfection made?

The causes which brought about this advance are numerous. In studying the history of early Greek art we can discern the following moving powers which account for the course it took and its slow or rapid progress. As a *primum movens* there was the creative instinct of man just alluded to. If this were the only moving power, unaffected by surrounding circumstances, unmodified in its course and progress by other active currents, the attempts of the later generations who start with the results attained by their predecessors before them and the experience gained by much struggle transmitted to them would show a constant advance upon the work of their fathers. But life does not present so simple a progression. There are other currents which either retard or accelerate progress. Among those we notice as a retarding side-current the law of inertia, which applies to man as it does to nature. In this case it means the force of habit and custom which leads men more or less voluntarily to resist the acceptance of new forms differing from those which they have learnt to admire, and which they associate with things highly esteemed. This conservatism extends not only to the appreciation of form but also to the older traditions of handling the material which we often see survive in cases where a new material suggested and required a new handling.

As a side current which accelerated the progress of Greek art towards freedom in the rendering of nature we notice among others, that certain changes in the political life of the ancients favoured the free development of art. Such were the accession to power of splendour-loving princes like Polykrates of Samos, Hieron of Syracuse, and, above all, Peisistratos and the Peisistratidæ of Athens. The most important political event was the Persian war and the wealth and glory it brought to the Greeks. We must further notice the important influence which technical inventions exercised upon the development of art, as the invention of the sawing of marble by Melas of Chios and his school, which freed the early wood-carver

from the restrictions of a round stem of a tree narrowly circumscribed in its dimensions. The art of soldering iron, discovered by Glaukos of Chios, which gave similar freedom to the metal-worker, and, above all, the invention of the casting of bronze by Rhoikos and Theodoros, which enabled the sculptor to bring his finished statue closer to his own clay model, reacted upon the art of modelling and did much to free art from its conventional trammels.

Again, the influence of kindred arts upon one another advanced each one in turn. So architecture advanced sculpture in forcing the sculptor to give freedom of movement and variedness of attitude to his figures, prescribing to him a limited space which he had to fill with his figures. A triangular pediment, high in the middle and low at the angles, was filled in the earliest art by quaint figures that grew smaller and smaller in dimension as they approached the angles. In order to avoid this absurdity, the sculptor, while placing figures erect in the centre, was forced to vary the attitudes from a slight inclination down to a completely reclining posture in the angles, and thus learned to represent figures with freedom of movement. The influence of sculpture upon painting and of painting upon sculpture furnishes us with reflexions which could hardly be dealt with thoroughly in only one address.

But of all causes which led the conventional early artist to nature, by far the most effective was the influence of the athletic games and the palæstra, which forms the subject of this address.

We shall see then, first, how the palæstra led Greek artists to nature, and secondly, how it led them away from nature to the ideal, or rather, through nature to the ideal.

It has often been said that a question clearly put is half the answer. In the present case to appreciate the influence of the palæstra upon the development of Greek art we must resolve the question into three definite ones, which will all tend to explain the influence exercised by the athletic games. At the same time these questions, if answered, will account for the peculiar rapidity of the advance made during a relatively short period of Greek art from conventional archaism to free naturalism and idealism, a fact, which, it appears to me, has heretofore not been satisfactorily explained.

1st. If we study the Homeric poems so far as they are concerned with plastic art, we must feel that they manifest the highest feeling for nature and freedom of execution in the rendering of the human form. This has already been felt with regard to the descriptions of the works of art the poet saw in his mind's eye and described as if really existing. But it appears to me that the most important side of the artistic feeling in the Homeric poems is to be found in the description of gods and heroes, and, more especially, in the sensuous description of the warriors in action and the careful study of the anatomy of the human figure as shown by the account of the wounded and falling heroes. In Homer's picture of a spear-hurling warrior, we have before our mind's eye the perfect statue of a spear-thrower;

when the hero seizes a rock, we have before us a diskobolos; when with "loosened knees" the wounded combatant sinks to the ground, we see a figure from the Ægina pediment or a dying Lapith or Gaul. Now, with such feeling for form, such definite conception of what is sculpturesque, so clear a notion of the parts of the human body, the step to the actual execution in the sculptor's material is but a small one. People possessed of such sense for plastic composition cannot remain content with mere symbolical natureless art, and the technical means of expressing their inner wants are a matter attained with comparative ease. Yet the fact remains that even for centuries after Homer thus manifested his sense for what is sculpturesque in nature the extant statues are stiff and conventional and do not manifest even approximately the feeling for nature which we find in these early poems.

2nd. If we examine the earliest monuments which we may attribute to the eighth century, and compare them with those belonging to the middle of the sixth century, we find that they are comparatively on the same level of imperfection as regards their want of freedom and nature, and we must be struck by the relatively small advance made in two centuries.

3rd. Now, with this hardly noticeable decrease in stiffness and conventionality during centuries, we must be struck by the singular phenomenon that the advance from the imperfect and unnaturalistic to the most perfect freedom in the rendering of the forms of nature takes place within a period of fifty years, from about 510 B.C. to 460 B.C. Within this short span of time the gulf which separates some of the stiff statues like the Apollo of Tenea from the Diskobolos of Myron, some of the lifeless seated figures of the Branchidæ from the early works of Pheidias, is overleapt. How could fifty years create so great a change as is shown by the comparison of the Apollo of Tenea, devoid of nature, with the Diskobolos of Myron, who is breathing with life? Other works of Myron are described by ancient authors as true to nature even to deception. The statue of the runner Ladas is called breathing with life (ἔμπνους); the runner seemed, it is said, with his last breath, to jump from his pedestal to grasp the victor's wreath. Centuries after Homer had in his written descriptions manifested such a keen sense for nature in the rendering of the human form, works as conventional as the Apollo of Tenea were produced, differing but slightly in their lack of naturalism from the Apollos of 160 years before. Fifty years sufficed to produce all the freedom and nature in conception and execution of sculpture, which we can estimate when comparing the Diskobolos of Myron with the Apollo of Tenea. "How can this be accounted for?" is the question to be answered.

The explanation which has until now been given, is that the Persian wars and the Greek victories produced a favourable change in the life of the whole Greek people, political, religious, and intellectual, and also freed art from its conventional trammels. Now,

there can be no doubt as to the important influence which the Persian victories had upon the development of Greek art. Yet I have always felt that while it to a great extent explained the loftiness and greatness of character which marks the art of the age of Pericles, it does not sufficiently explain the definite advance made in art from more abstract conventionality down to nature. Moreover it took some time before the great spirit bred by the heroic efforts of the Persian wars had so far transfused the whole nature of the people as to impress its spirit upon so special a manifestation of the human mind as art. And so it is not until the great works of Pheidias which group round the year 450 B.C. that this spirit is exhibited in art; while the actual advance from conventional archaism to freedom and nature takes place in the years immediately preceding and following the Persian wars. The really efficient cause of this rapid development can therefore not be found in the influence of the Persian wars, but lies in the growth of athletic games and the systematic development of the palæstra, especially when these are brought into an immediate relation to art.

Why Greek art should not have advanced more rapidly towards nature during the centuries preceding the "period of transition," is explained by one simple fact, namely, that before this time the statues were almost exclusively religious. The early wooden statues, the *ξόανα*, were statues of gods forming an important part of religious worship. The tendency of religious worship is naturally conservative. The very earliest images of the iconic period * were necessarily rude. The more remote the age of such an image, the greater the mystery attached to it, the greater its religious weight. The earliest statues were considered by the Greeks to have fallen from heaven (*διοπετής*). The more a statue was like these holy early images, the greater its sanctity, and thus there would be a natural tendency to repeat the earlier types. Nay, even later, when freedom and nature had gained their sway over art, the gods, when grouped with heroes, are more conventional in treatment than the less divine beings. The same holds good in all periods of art. I need but remind you of the solemnity, bordering on severity, of the religious pictures of Bellini as compared with the perfect freedom of conception and execution manifested in his small pictures with classical subjects in the Academy at Venice. So long as the sculptor's art is entirely in the service of religious worship, there is small chance of its forcing itself from conventional imperfections. To be brought down to nature the sculptor must be brought down from the gods, face to face with man, and then he may reconstruct his gods out of the ideal combination of the most perfect forms which he has studied in man, but has never found together in one man.

In the first place the palæstra led the Greek artist down to man.

* There was an *aniconic* period of Greek religion in which gods were worshipped, not in actual images, but in objects and localities of nature, and in symbolical structures suggesting human form.

It was here that the Greek people and the Greek artist had their feeling for the human form, its manifestations of strength and perfect proportion, aroused and developed. In the athletic games, to which a moral, nay even a religious importance was attached, victory, which brought glory to the victor and was the pride of his community, was based upon the perfection of the human body, the force and normal development of all the organs, flexibility and dexterity of movement, which the early artist failed to render in his statues, and with regard to which the sense of the public at large seemed comparatively blunt. It was here, with hundreds of nude youths, not only wrestling, jumping, and running, but endeavouring by systematic practice to remedy any defect or abnormality in any one limb or organ, that the artist day by day studied his anatomy of the human figure without the need of entering the dissecting-room or calling in the help of the anatomist. And when once the artist was called upon to commemorate by means of his art the outward form of the athlete whose perfect development gained him the glory of victory and monumental fame, we can then see how the sculptor was led away from the conventional archaic types of gods down to nature in living, active, and well-formed man.

All this more or less *a priori* reasoning makes it most probable that the palæstra was the most important agent in bringing Greek art down to nature in the fifty years marking the "period of transition." An actual examination of the facts and a careful study of Greek art with this question before the mind, give the most conclusive evidence of the supreme influence exercised by the games and the palæstra. I cannot hope in this short address to place before you all the instances bearing upon this subject which I have collected for the last four years, and which have shown me conclusively that we must ascribe to the palæstra the chief influence in freeing Greek art from its conventional trammels. On the other hand I do not mean to appeal to your faith in my personal statement; but I believe that the instances which I am able to place before you in diagrams and casts will suffice to illustrate and support the points to which I shall draw your attention. Still I feel bound to inform you that the choice of these special instances has often been guided by mere convenience and readiness of access, and that in many cases, as with some of the vases, the diagrams were made for other purposes; and thus it cannot be said that I have chosen but a few instances happening to prove my generalisations.

From the most general point of view, we must be struck by the fact that the Greeks, the one people in antiquity whose art is possessed of nature, were also the one people with whom athletic games were a national institution, wide spread and part of daily life. I have endeavoured to show elsewhere how this fact as well as the plastic predisposition of the Greek race was a necessary outcome of the fundamental characteristics of the race and their physical and social surroundings, and to point out that Oriental nations and those living

in a tropical or northern climate could not develop the same characteristics. We must at least note the fact that the two distinctive elements of Greek life, a high development of athletic institutions and of naturalism in plastic art, are found together.

But what tends more directly to show the immediate influence of the athletic games and to solve the main question we have placed before us, is the fact that the fifty years which mark the transition from conventionality to freedom and nature in art are also the years in which the athletic games became really elevated to the important position which they occupy in our mind, and which they did not always hold; that in this period the palæstræ or athletic schools became real national institutions, thoroughly organised all over Greece.

As with art and most higher manifestations of human thought and culture, the early stages are almost always essentially religious in character, so the athletic games in earlier times were either associated with some worship of god or hero or were part of the funeral ceremony, thus partaking of an essentially religious character. Towards the close of the sixth century the great games, such as those of Olympia, partake more and more of a national and political character. They become the central point of peaceful union for all Greek states. The increase of their national importance sprang from the growth of the feeling of Panhellenic unity which preceded the Persian wars; yet they no doubt reacted strongly upon this feeling, and served to bring together the people of the various states, and to make them feel the common bands which bound them together. The political importance of the great games, especially those of Olympia, can hardly be over-estimated. This political importance was, no doubt, felt by Peisistratos, who, along with Pericles, was the greatest of Athenian statesmen. He appears to me to have foreseen the greatness of the future of Greece, and above all, of Athens. On the model of the Olympian games he revived the Athenian games, and as there he traced the growth of Panhellenic feeling, so here he wished to create a real Panathenaic feeling. He added new games to the old ones, gave greater splendour to them, and, as the Olympic games recurred at periods of four years, determining the computation of time for the whole of Greece, so he introduced the Greater Panathenaic, recurring every four years and determining the computation of time for Athens. It is a noteworthy fact that every great political leader in Athens marked his political activity by some addition to the Panathenaic festival. After Peisistratos, with the Peisistratidæ and with Pericles, the games were further enriched and obtained still greater influence. Furthermore, we must attach the greatest importance to the development of the palæstræ or athletic schools during this period. By degrees these institutions are established or rendered more systematic in their organisation throughout the whole of Greece, and become the schools for the physical training of the Greek youth destined to provide strong and active warriors to defend their native country.

Nay, they become the home for general education, where even intellectual training is carried on, and the philosophers form their circles of eager learners. As I have said before, it is here that the artists studied the human form in rest and action. It is here that the systematic training of each organ of the human body brought home to them the plastic anatomy of man, and that in the *σκιαμαχία*, in which the various stages of each game were gone through, the sculptor had impressed upon his eye in living statues the typical attitudes of each game.

A still more direct proof is to be found in the fact that in this period the custom arose of commemorating athletic victories by statues. And now, as I have said above, the sculptor is brought face to face with man, and must bend his art and craft to the service of actual nature. According to Pausanias the first statues set up to athletic victors were those of Praxidamas and of Rhexibios, who were victors in the fifty-ninth and sixty-first Olympiads, that is, about 530 B.C. They were of wood, and, according to his description of the one to Arrhachion, were very similar to the statue of the Apollo of Tenea. The influence of the palæstra and the introduction of the custom of erecting statues to victors did not take immediate effect, or at once convert imperfect art to a state of perfection, but it was inch by inch that conventionality strove to maintain its ground, and step by step that art advanced towards nature within this comparatively short period of fifty years. So we can see in the extant statues the gradual growth of freedom and the falling away of the archaic fetters. In these three instances we have the chief stages of this progress. In the Apollo of Tenea at Munich, in attitude, in the composition of the parts of the body and in the modelling of the surface, we have hardness and woodenness far removed from the actual appearance of the living organism. In the so-called "Strangford Apollo," in the British Museum, in whom I see an athlete belonging to the school of Ægina, we have a great advance in the direction of nature. Though the attitude is still conventional, the feet placed one before the other, the arms pinned to the sides, the head straight forward at right angles to the chest, the limbs seem joined more organically to the body, and, above all, the surface is modelled so as to present a continuous rise and fall, not an abrupt succession of ridges put together, and to suggest the various organs which it covers. Still, though the growing feeling and desire for rendering nature is manifest in this work, we notice a struggle in overcoming the difficulties presented by the material. The traces of conventionality are but very slight in this third statue, the Choiseul-Gouffier Pugilist, formerly known as an Apollo. This work is most probably the work of Pythagoras of Rhegion, a sculptor who stood on the very border line between dying archaism and the vigorous life of fierce naturalistic art. Here freedom is given to the attitude (a typical one in a certain stage of boxing): the athlete rests upon one leg more than upon the other, the arms are freely extended, and, above all, the

urface is modelled with a perfection which presents most vividly the lexibility of the human skin and the change of surface as it covers organs of different form and texture. Finally, in the Diskobolos of Myron we have fullest freedom of attitude in the indication of active ife and in the modelling of the surface. The artists had to exercise heir power of rendering the life of nature in many an athlete statue efore they gained the full benefit of the growing influence of the alæstra. It was chiefly in the schools of Ægina and Argos that his training was undergone. If we but bear in mind that, before he year 530, statues were only of gods and there existed none of thletes, and compare the enormous preponderance of athlete statues over those of divinities with the sculptors of Argos like Ageladas, of Ægina like Kallon and Onatas, of Athens like Myron, we can realise he actual influence which the growth of athletic institutions had upon art. I would beg you to connect in mind with what we shall earn concerning the ideal period which followed this step down to nature the fact that after this period of transition, especially with the great artists of the Attic schools, there was no such preponderance of thlete statues over those representing mythological subjects.

Finally, in carefully studying the extant ancient monuments, we realise the great and direct influence of athletic games, while at the same time we explain a fact in the early Greek which has often been noticed, and never, to my knowledge, satisfactorily explained. It is the fact that, down to the time of Pheidias, the treatment of the nude male figure far surpasses in perfection the rendering of the head, which is hard, lifeless, and conventional. This shows that the body engrossed the artistic interest and attention of the sculptors, that this appreciation of the human body is to be attributed to the engrossing interest of the athletic games, and that the palæstra was the real school for the sculptor. Look at the feeling for nature in the body of this "Strangford Apollo," and compare with it the lifelessness of the head. You must be struck by the exceedingly careful rendering of the human structure—limbs, muscles, sinews, and surface—in the figures from this pediment of the temple of Athene at Ægina; then compare with it the lifeless conventionality of the heads. In the same way it is the influence of the palæstra, which, arousing the interest in the male human figure, and giving the sculptor the power of rendering it with truth to nature, accounts for the inability freely to render the female figure, as compared with the great skill with which the male figure is represented during this period. Compare the nude warriors from this same pediment with the Athene in its centre, and the difference will be most manifest. And, thirdly, this supreme influence which the athletic games exercised upon artistic feeling and upon artistic creation is shown by the fact that, while during this period the modelling of the nude male figure is so perfect, the modelling of drapery is still in its elementary stages; so that even in the works attributed to Kalamis, the older contemporary of Pheidias, the modelling of the drapery is comparatively hard and

conventional. Still, it was the palæstra that led the artist down to nature, and if it naturally be above all in the nude male figure, the central task is still accomplished, and the extension of this attainment to other "unathletic" objects is a necessary sequence. For it is a well-known truth, felt by all artists, that whoever has the power of drawing or modelling accurately and with truth to nature a nude male figure, can render with the same correctness whatever he sees before him, provided it engrosses his interest and occupies his attention and practice for some short time.

We have now seen how the first great task in the development of Greek art has been accomplished chiefly through the agency and influence of athletic games. The artist has been brought from conventionality and the abstract symbols of gods down to nature and man. The next great task, as we have before put it, is to lead the artist away from nature, through nature to the ideal. There really was some danger that Greek art would not rise higher than the mere accurate rendering of nature, which would lead to extreme realism, and, through the final stage of overdone technical skill, to a speedy degeneration. In Myron there is evidence of this danger. His statues, such as that of the famous cow, were praised for their extreme realism almost leading to deception; and in the excessive movement which he put into some of his statues, such as that of the Diskobolos, even according to the testimony of the ancients, there was an element of sensationalism which, if it had swayed Greek art, would have led to a rapid decline. In order that Greek art should ever reach the height which it actually did, and which, as I pointed out to you at the beginning, is the cause of its persistency of influence even down to our own times, it must add the ideal to nature gained—establish the *natural ideal* of the human form.

There can be no doubt that in the fulfilment of this second great purpose the heroic spirit of the past Persian wars and the enlightenment and culture of the Periclean age were most effective. It predisposed the people towards the appreciation and accomplishment of great works, raised them above the sphere of mere individual interest into that of great common purposes and aspirations, gave them that characteristic feeling for width and grandeur, the real and most eloquent expression of which is to be found in the art of Pheidias. Yet in a more direct and immediate way the palæstra was again instrumental in leading art to make this great step.

If the artist has been led to appreciate and study nature and to endeavour to render it accurately in his works, this may lead him at last to imitate most minutely what at the time he sees before him. This may be excellent practice; but it will never create great art. For the individual man is imperfect, and the rendering of those imperfect forms will not satisfy the feeling for law, order, harmony, or design inherent in the human mind, the primary impulse to all artistic creation.

The palæstra was the real school for the Greek artist: here he

spent his time and studied the human form; but not only in individuals. Constantly from his earliest youth, day by day, he had before his eye numbers of well-built youths in all attitudes and all actions, and these series of individual forms impressed themselves upon his mind until they became an intrinsic part of his visual memory and imagination, forming, as it were, an alphabet with which he could create at will things of great and new meaning. Just as letters, words, and grammar have become to us elements and units of thought which lie ready to be composed, without effort, as far as they are concerned, into phrases, sentences, periods, books, poems and orations with great and new meaning and perfect form, so the existing human bodies and their changes in various attitudes and actions became such elements to the visual and imaginative mind of the ancient Greek artist. They did not require conscious attention, but became the parts of a great and new composition, with a meaning and spirit as a whole, lofty and high, yet ever intelligible, because composed of these elements familiar to man from the daily suggestion of nature.

It is therefore that the human forms which they present are so perfect. Turn to an entirely different period of art and you will notice a similar phenomenon having similar causes. Northern renaissance painting and sculpture, which possess so many great characteristics of their own, widely differ from Italian renaissance art in that their renderings of the nude human figure are devoid of the grace and harmony which those of the Italians possess: they are to a greater extent the portraits of individuals whose bodies were not perfect in all parts. This is chiefly to be attributed to the fact that in the southern districts partial nudity is more common, and from an early age the Italian has more than the German or the Fleming to some degree reduced the individual human forms to an alphabet, and has created more ideal individuals of art. Most of the nude subjects in modern works of art are really studies, and not pictures or statues. The modern artist depends upon his one model, and if the figure be more or less perfect we are rather inclined to praise him for his skill or luck in choosing a good model than for his artistic imagination.

This feeling for perfection of form which the Greek artist gained through the study of so many individual instances in the palæstra soon became conscious, and the attempt was soon made to find the most normal proportions of the human figure as suggested by the palæstra. The generalisation from the individuals to the ideal type was bridged over in the palæstra itself by certain classes or groups of individuals forming types of their own. As the palæstra became thoroughly organised in this period, so the games were classified and systematised, all joining in the one great end of producing healthy and strong youths serviceable to the state. Thus the games were subdivided into light and heavy, *κοῦφος* and *βαρύς*, having their particular type and fullest representatives in certain classes of men.

Running, jumping, and throwing the spear were typically light games; boxing and the pankration were typically heavy. The pentathlon stood between both, and, uniting several kinds of games, had for its aim the production of a normally built agile man. Other subdivisions might thus be pointed out; but they tended in themselves to drive the artist towards the establishment of normal types for each part of the body and for the proportions of the parts. Thus even in the early period at the beginning of the fifth century, Ageladas of Argos, the teacher of Myron, Pheidias, and Polykleitos, wrote a treatise on the proportions of the human figure. Polykleitos of Argos and Lysippos established a canon of the human figure of which we hear that the one was square and massive, the other slimmer and lighter with smaller head. We must not lose sight of the fact that these canons of perfect human proportions were not at once represented in an Apollo or a Hermes, but that the Doryphoros and the Apoxyomenos are athletic statues. Those canons have been identified in these two extant statues, a marble copy of the Doryphoros of Polykleitos in the National Museum at Naples, and a copy of the Apoxyomenos of Lysippos in the Braccio Nuovo of the Vatican. They both have the characteristics ascribed to these canons by ancient authors.

In such canons and ideal types we are raised above the individual to beings higher than man. Having been led by the palæstra down to man, the artist can now rise up to the gods with a new ideal of divine form, for the human form that is above existing man in its perfection and still is possessed of human qualities brings the artist face to face with the figures of Greek mythology.

This influence which Greek athletic art, when once it was established in all its truth to nature and ideal breadth, exerted upon mythological art, is most clearly shown in extant monuments.

It is really only after the period of transition that the types of the various Greek gods as we know them become fixed and developed in art, and for a long time we can then trace the immediate influence of the palæstra in these statues of heroes and gods.

After Polykleitos had established his athletic canon, in all the works attributed to him and his school, whether athletes or gods, nay, even in female figures, we can see the characteristic proportions of the Doryphoros repeated. In later times, in the works of artists that are not the direct offshoots of his school we can often notice the influence of the athletic type of Polykleitos retained or even revived in the figure of some god or hero. No doubt some gods and some heroes are more adapted by their character to assume the form of such a canon. So it is especially gods like Ares and heroes like Herakles that from their nature are square and massive, and are thus properly represented in the form of the "quadrata signa" of Polykleitos. In the numerous mythological figures manifesting the Polykleitan canon that have come to my notice, there have been instances in which I at first thought they were athletes, and then found they were mythological figures; and some in which, for example, I thought the

statue was a Herakles with the apple of the Hesperides strongly representative of the Polykleitan type, and then found that it was really an athlete with an oil-flask. This I mention merely to show you how through the establishment of these athletic canons the representation of mythological subjects was influenced. The same must be said of the influence of the Lysippean canon which can be traced in so many works, not only those immediately of his own school. As the Polykleitan canon, from the nature of its proportions, was best suited to the heavier and more muscular gods and heroes, so the Lysippean canon corresponded more to the lighter and fleeter gods. Nay, even as late as the last half of the first century B.C., in the school of Pasiteles, we can notice the influence of athletic art upon mythological art in the new eclectic canon which that artist and his school established, represented most fully in the statue of an athlete in the Villa Albani by Stephanos, the pupil of Pasiteles, and traceable in all the mythological statues of that school.

It is, however, not only in statues that this influence of athletic art upon mythological art becomes manifest. By far the most curious and interesting instances of this influence are to be found in the minor arts, especially in vases. I have collected a large number of such instances on vases with athletic scenes. We find there how, following the normal course of the Greek mind not only in art but also in religion and literature, the Greeks construct their mythical and heroic conception upon the basis of real life. Thus their representations of real contests in the palæstra form the groundwork for the representations of similar scenes in the mythical and heroic world, as their religious idealism drove them to establish the ideal prototype for all actions of real life. When Pindar commemorates in an ode the victory of some living athlete, he generally begins or ends with some typical contest from the heroic world related to it. In the same way the vase-painter, when painting a prize vase or one decorated with athletic scenes * places a scene from the actual palæstra on the one side, and on the other some similar mythical scene, a religious type of the game. Prominent among such types are Theseus and the Minotaur, Herakles and Antaios, Herakles and the Nemean Lion or the Lernean Hydra, Peleus and Atalanta, contests of Trojan heroes, and many others. Now the form given to these mythical contests, to which I shall have occasion to draw your special attention, is the same as that in which the real scene is put, or rather presented itself to the artist while studying his art in the palæstra. So constant did I find this arrangement to be that it almost partook of the nature of a law and gave the observer the greatest gain of science, the power of prophecy.†

* Except in the cases of Panathenaic prize vases, where one side always contains the conventional figure of Athene, like the Athenian coat-of-arms.

† It appears to me that the study of the destination of a vase is often of the greatest use for the interpretation of the vase painting. Thus a sepulchral vase, or one meant to be a gift between lovers, or a drinking cup, would be decorated with mythological scenes appropriate to its original purpose; and often a typical

I cannot resist relating to you an instance illustrative of this result. While studying the vases in the Louvre with the question of the Influence of the Palæstra before me, I mentioned to one of the officers who kindly assisted me in my work this result of my observation of athletic vases. "Do you mean to say," he inquired, "that, seeing the athletic side of a vase, you can tell what class of mythological scene will be on the other, or what class of mythological scene on the one will have an athletic subject on the other?" I answered that I believed I could tell what scenes would probably have such correlatives. "Let us see," he said. We proceeded to a room I had not yet examined, and, passing a glass case by the window which contained pateræ and κύλικες, I noticed a κύλιξ, a drinking cup with the shape of a flattened bowl, so placed that the convex outside, ornamented by a broad border with numerous figures, was uppermost. The subjects represented on this border were scenes from the contests of Theseus and Herakles. "If there is any representation in the inside of this vase," I said, "it is most probably an athletic scene or figure." The case was unlocked, the vase taken out, and, when turned round, the centre contained a round medallion of yellow ground within the black patina of the vase, and, there being only room for one figure, it displayed a youthful athlete with halteres in his hand and a discus by his side, evidently a victor in the pentathlon.

So direct was the process by which the vase-painter transferred the scenes he actually saw and studied in the palæstra, that we can trace even in the details of the figures composing such a mythical scene their athletic origin.

The simplest and earliest form of an athletic scene is that shown in this diagram, copied from an archaic black-figured vase. In the centre we have the two combatants; on the one side we see a nude athlete in a peculiar attitude, with arms drawn up, recurring in almost all representations of this kind, the odd man in the game, the Ephedros, waiting his turn. On the other side there is an older bearded figure, draped with a mantle, and with a long staff in one hand, the Paidotribes or Agonothetes, the teacher or umpire. Now, in the mythical scenes the vase-painter places his heroic or divine combatants in the same way in the centre, and on either side he places divinities as judges, spectators, or protectors. The vase-painter takes the Paidotribes, draped and with long staff, simply alters the head of the bearded man to a female head, the staff to a spear, and sometimes adds indications of the Gorgoneion on the breast, and we have Athene. On the other side he retains the nude youth only, placing in one of his up-drawn hands a short caduceus, and we have Hermes. To revert once more to sculpture, we find that the central figure in the Ægina pediment, presiding, as it were, over the contest for the body of the fallen Patroklos, is the same Paido-

scene of mythology was modified to suit the character of the vase. See the paper by the present writer, on "Pythagoras of Rhegion," &c., Journal of the Hellenic Society, vol. i. pp. 184–5 (footnote), 1880.

tribes-Athene that we meet with on these archaic vases. The habit of building up the scenes of mythical contests upon the actual scenes of the palæstra was so strong that sometimes the vase-painter forgets and betrays himself in mixing up in the same mythical scene athletic and mythical elements. This archaic vase-picture from an unpublished small Lekythos in the Louvre is an instance. The mythical combatants are here Theseus and the Minotaur; yet the vase-painter, unconscious of the absurdity, places on either side two real nude athletes in the attitude of ephedroi, as if awaiting their turn to enter a boxing match with the monster whose head is being cut off by Theseus. Peaceful contests are directly translated into armed struggles. The metopes of the Parthenon and Theseion, the frieze of Phigalia, show innumerable instances in which Theseus and Herakles struggling with monsters, Lapiths slaying Centaurs and Amazons, are represented in the typical attitudes and actions belonging to the palæstra and studied there by the artist.

These observations are not restricted to statues and vases, but apply equally to the minor arts, such as that of the die-sinker and gem-engraver. Coins manifest, far more, I believe, than has until now been recognised, this immediate influence of the athletic games. Mr. C. T. Newton and Mr. R. S. Poole have shown how in the coins of Syracuse and Camarina even individual victories are recorded. I shall merely point to one instance. This first coin of Selinus in Sicily, the date of which is about the first half of the fifth century B.C., represents the river-god Hypsas as a nude youth with a patera in the one hand and a short branch in the other. As I have once before endeavoured to show, the type of this figure, though reduced on a small coin, corresponds in proportions, modelling, and attitude to the Choiseul-Gouffier Pugilist whom I attribute to Pythagoras of Rhegion. About 430 B.C. (observe that the Polykleitan canon had come in) the same coin with the same river-god changes. As you see, the type, the proportion of the figure becomes squarer and more thick-set, the one leg is drawn back, the weight resting almost entirely upon the other (*uno crure ut insisterent* is the characteristic attitude ascribed by Pliny to the statues of Polykleitos), the branch is elongated and carried more like a spear—in short, the figure approaches most manifestly the type of the Doryphoros drawn on this diagram from the Naples statue.

If time permitted, I could bring before your notice instances to show in the same manner the influence of the athletic games upon the works of the other minor arts, such as gems and terra-cottas. Yet if we have shown it in the greater arts, this also demonstrates it for the lesser ones. For the minor arts of Greece, as has become evident from so many instances, took their types and models from the great works; often one and the same artist created both, and thus the influence detected in the one applies by implication to the other.

I hope that I have made you realise the important part played by the athletic games in the development of Greek art. It must, I believe,

be recognised that those qualities which distinguish Greek art at its height, the combination of Nature and the Ideal, were given to it chiefly through the influence which the athletic institutions and their growth and development had upon the people at large and the artistic world in particular. In all branches of science it has been found that while certain broad currents of influence in the formation and development of a growing organism could be perceived and traced during the earlier and similar phases of its existence, or rather growth, this is more difficult, indeed hardly possible, when once the body, genus, or institution has reached the more complex and variegated forms of full organisation. All the conscientious observer can then do, is to record the parallelism and concomitance of the development in the two forms which before held the relation of influencing force and the thing influenced, showing a certain relation that exists between them or which they have in common to some other force, and, in many cases, leaving the question open which is the cause and which the effect.

So in the present case, after having found that the palæstra was efficient in bringing Greek art to its full development, we still have evidence of an intimate relation existing between art and athletics. Yet we cannot always say where exactly the influence lies, which is the institution influenced, and which is the one influencing. All we are bound to do is to record the parallelism in their development.

The broad lines which mark the development of athletic institutions are the same that characterise the development of Greek political and social life, Greek literature, religion, and art. In political and social life we have the undeveloped earlier forms of small communities leading in the great age to the Panhellenic unity in which all Greek states felt the common ties that united them and in so far submerged their own individuality. More and more this great and broad conception of state which animated the Greeks gives way to the growing assertion of the interests of individual states clashing with one another. Further, in the same state even party feeling asserts itself in opposition to the state, and within the party again, the individual seeks his own good to the detriment of the party. Gradually, step by step, with here and there a short flickering up of the great spirit in a different form, the dissolution of Greek unity leads to the final destruction of Greek independence. With the decline of political grandeur, strength and virtue die in the social life of the Greeks. The old simplicity and greatness of character decay, and dissoluteness and vice more and more take root and undermine the moral strength of the people; for in no time and in no country were political and social ethics so dependent upon one another. In literature, especially in the history of the drama, we can notice the same step from the more conventional forms, traces of which are still to be noticed in Æschylus, to the more minute individualisation in Sophocles and Euripides, sensationalism already beginning in the latter. Art was so complete an expression of Greek religion that in studying the development of art we can follow the course taken by religion. From the first

stage, the conventional archaic art, we arrive through the period of transition to the great art of the Panhellenic period. The spirit of the religious art of this period is to be found not only in the character of the work, the style of the artistic schools, but also in the subjects represented. The great gods Zeus, Hera, Athene, Demeter, are the subjects most commonly represented by the artists, and when they do represent the other divinities they give them a severe, large, and noble character. Such is the case with the severe conception of Aphrodite, Apollo, Artemis, and Dionysos. After the great age, as in politics so in art, the step is made from the ideal down to the individual again, from god to man, from the type to the portrait of living individuals. At the same time in religious art, the great gods no longer form the chief subjects represented by the sculptor and painter; minor deities, nay, mythical personifications of lower nature, such as fauns, satyrs, mænads, are now represented by preference. In the gods again the most human side is emphasised; in Aphrodite and in Apollo, who is made younger and younger, the more sensuous and less divine side of their beauty is made prominent, until in Kephisodotos the younger, when the fourth century laps over into the third, sensuousness merges into sensuality. The grave and noble simplicity of the ancient religious art expires with the decline of religious faith in the gods and the dissolution of national greatness, and in the school of Pergamos and Rhodes the dramatic and sensational phase of sculpture prevails. In this period we pass from *genre* sculpture to the comic, the grotesque, and the brutal.

The palæstra follows the same course. In the first and earliest stages of the palæstra the athletic games are not completely organised; they have not yet established a character of their own, but are a class of religious institutions without the human life and interest which they gain when once they are brought down from gods to man. From having been religious, they must, in the great period of their development, which coincides with the great period of political and intellectual life, become a national institution. This step is made during the period called in the history of Greek art the period of transition. In the highest period of the palæstra this institution has a real national aim, to provide and encourage perfect physical education for the youth and men who are to form the strength of the nation. It is a noble aim, and, throughout, the character of the great games and of the palæstra is of the wide and lofty nature which stamps itself upon its artistic productions, and thus affects the spirit of art. The statues in honour of athletic victors are broad, large and monumental in character, in subject, and in execution. An individual victory is not commemorated by the portrait-statue of the victor, but by a perfect type of that class of athlete and that game. It is that which lasts when the individual passes away, just as in the representation of gods during this period all that is ephemeral and individual in mortal life was avoided. So too, in the execution of the works, the transient and sensational is shunned. The attitudes are restful, however great the life and the

suggestions of active vitality may be; there are no sensational momentary poses; the modelling is broad and large, without any of the tricks of craft and the display of technical skill which distinguish the later works.

In the next period, marked in art by the growth of individualism and the gradual spread of sensuality, the palæstra is marked by the most pronounced individualism and the introduction and spread of professional athleticism. As the palæstra grows in importance, and as the rivalry between the various states grows hotter, the interest in the individual victor and his importance grow, and thus in the age of Alexander the Great and of the sculptor Lysippos, when in other spheres the feeling for personality ran higher, the custom is for the first time introduced of erecting portrait statues in honour of athletes who vanquished three times. Before this, it was considered sacrilege to place the portrait of living persons on public monuments, as is evident from the charge brought against Pheidias for having given his own likeness and that of Pericles to two Lapiths in the Amazon battle represented on the shield of the Athene Parthenos. When once this innovation is introduced, towards the close of the fourth century B.C., the public character of such monuments makes these portraits a bridge over into more ideal arts. It will readily be seen what influence this custom of athletic art must have had upon the arts of portrait-painting and portrait-sculpture, and how this directed the course of art towards realism.

This greater realism is also to be noticed in the attitudes and poses given to athlete statues, more momentary and less monumental than they were in the great age. The same causes which led to the growth of individualism effected the great change in the spirit of athletic institutions. While before they were a means to a great political and social end, they now become ends in themselves to which all other considerations become subservient. While before athletic exercise was a part of the daily occupation of the Greek youth, which was meant to contribute its share to the great end of making him a sound and normal being, harmoniously developed both in mind and body, and thus a serviceable citizen to his state, it now, step by step, becomes itself the great aim to which time, life, and aspirations of the youth are devoted, and to which they are made subservient. It is the step recurring in the history of athletic games in all times, the step from the gentleman athlete to the professional athlete. In art we see the signs of the loss of proportion in such works, which increase in the next period. We hear from ancient authorities how pugilists and pancratiasts were fattened up and made bulky, how muscular development was exaggerated even to ugliness. In the mythical figure most immediately influenced by athletic art, in Herakles, we see this in later instances, where the muscular development is abnormal and repulsive. The germs of the rapid decline of this great institution are to be found in the fungus growth of its own importance, growing till it obscured the great aim which gave it life and characterised its

highest development. It leads to degeneration or, as the pathologist would more accurately term it, to hypertrophy. Let me only bring before you one interesting instance to illustrate this step towards professional athleticism. This coin of Amyntas III. of Macedon, who reigned from 389 to 369 B.C., representing a horse with its rider, is typical in one respect of all similar representations before the middle of the fourth century B.C., namely, in respect of the relation of rider and horse and of the corresponding importance of both in the minds of the people of that time. Like all representations of riders down to the middle of the fourth century, the rider is here large in comparison with the horse. If now we turn to this coin of Philip of Macedon, there is a striking difference in this respect, the horse being disproportionately large, while the rider has dwindled down to an undergrown jockey. The whole matter is explained by the fact that this coin of Philip represents his racer, whom he sent to Olympia, and who there came out the winner. Now, in the previous periods it was for the rider's sake that horse-racing existed, it was to show and encourage his skill in horsemanship, and he got the glory; there existed no jockeys. In the time of Philip the horse became the great centre of interest, and the gentleman rider and warrior of the Parthenon frieze is no longer to be found at Olympia. In the course of this natural or unnatural selection, the horse too has altered its form, merely to excel in fleetness. It is curious to consider how similar the action of these "laws" has been in ancient and in modern times. Thus not only with the human form but even with animals the course taken by the athletic games in the later periods tended to destroy the ideal of form established, during the great age of Greek culture, by art through the earlier influence of the same institution.

In the last phases of the history of the palæstra we can distinguish three manifestations of the decline. Corresponding, first, to the dramatic stage in the history of Greek sculpture, which led to the groups of Pergamene and Rhodian schools, we have sensationalism in the games, encouraging wonderful feats of abnormal strength or skill, and in athlete statues, dramatic attitudes, boxers with arms upraised, wrestlers leaning forward with arms extended, and a development of muscles that remind us more of the dissecting-room than of the artist's studio. Secondly, the brutality, the germs of which we noticed in the previous period, now manifests itself fully. Instead of the noble grandeur of a Doryphoros or a Choiseul-Gouffier Pugilist, we have fleshy monsters who would be comic if they were not repulsive. The drawing of this figure is from a terra-cotta in the possession of M. Camille Lecuyer at Paris, and represents a pugilist with arms upraised, whose bull-head reminds us so much of the Minotaur that we may fairly doubt whether it does not represent a bull-headed athlete or the Minotaur turned pugilist. Another telling instance of this class is a bronze in the Cabinet des Medailles of the Bibliothèque Nationale of the same city. It represents a pancratiast thick and fleshy, with swollen face, arms upraised, in the act of kicking with

his right heel. Such representations are inconceivable in the time of Pheidias.

The history of the Greek boxing-gloves, the ἱμάντες, typifies and illustrates the three chief phases in the history of the palæstra, from its height to its decline. The earliest form were the μειλίχαι, which were to soften the blow to the striker and the one struck, and were thus subservient to the exercise. The second form was the ἱμάς ὀξύς, a leather thong wound round the hand, protecting the hand of the striker, but increasing the severity of the blow; this belongs to the period when professional athleticism was beginning to be introduced. The third form, marking the period of decline, the Græco-Roman and Roman age, was the brutal cæstus, garnished with leaden balls, which produced disfiguring blows, sometimes leading to death.

As in the decline of Greek religious art, when practically the faith in the great gods was shaken, we have the introduction of *genre* and comic elements, such as putti or little cupids carrying the attributes of the gods, the thunderbolts of Zeus, the trident of Poseidon, or the club of Herakles, so in the last stages of the palæstra, when its dignity had vanished along with that of the gods, we see the sacred games robbed of all solemnity and transposed into the comic *genre*, in the form of little cupids, undergoing athletic hardships in quaint mock solemnity and exertion. The diagram before you shows one out of a large number of late reliefs, with chubby children hurling the discus, boxing, wrestling, running, jumping, racing in chariots and on horseback. Here is one led away after a defeat in the *pygme;* here another miniature Diadumenos fixes the victor's wreath to its brow; here are chariots colliding and crashing asunder, horse and driver overthrown, and so on—all scenes of the great palæstra made quaint and comical. Surely all solemn or religious associations must have left the games when once they could be represented in this form. Such representations, too, are utterly inconceivable in the age of Pheidias. The real death of all great institutions has set in when once the ridiculous side is brought out. The most notable instance of this is the final death-blow administered to chivalry by Cervantes in 'Don Quixote.' When once the Greek games are made the subjects of these comic scenes their end is reached, and they die with the extinction of Greek political freedom and the fall of Greek literature and art.

I have brought before you the influence of the Greek athletic institutions upon art in the effect they produced, leading the Greek artist to nature and the ideal in the representation of man. This applies chiefly to the single figures in sculpture. There is one more great achievement of Greek art, in which it has supplied all ages with an artistic principle as fundamental as the ideal of the human form; and this again, I hold, is chiefly due to the influence which the athletic games had upon the development of Greek art.

The masterpieces of Greek painting have all been destroyed, and our information concerning them is derived from the numerous accounts of ancient authors, and from their feeble reflexion in the works of the

minor arts, such as mural and vase painting. Thus the common error is widespread that Greek painting was comparatively on a quite different scale of excellence to Greek sculpture. I have reason to hold that this is not so, and that, with the exception of landscape painting, the standard of Greek painting was comparatively as high as was that of their sculpture. However this may be, one fact remains, that they are the first to have established the fundamental principle of pictorial art, and that this was first done in athletic art.

This fundamental principle of pictorial art is expressed by the word *composition.* What constitutes a picture a work of art is the artistic organisation which the artist gives to the elements which he copies from nature. It is not merely a tree and a house and a man that make up a picture, but it is the combination of these elements into unity and harmony suggested and demanded by the feeling for and need of design inherent in the human mind. In our most complicated pictures we can distinguish the following elements of composition. First, linear composition, in which this unity is given by means of an outline to the whole drawing which meets in some central point; second, perspective composition, in which the representation of distance from the point of vision enables the artist to indicate the foreground and background with regard to the centre of interest; and in the third place composition is given to a picture by light and shade, the gradation of values of colours and of tone which give the same artistic unity within variety. But all these forms have this in common, that they impress upon the eye of the spectator a central point of design and interest, and that the other parts of the work lead up to it, making of the whole an artistic organisation with unity or harmony of design.

In the paintings of the East and of Egypt we have long successions of figures tier above tier relating in an imperfect language a scene as we should relate it in a succession of sounds called words. In fact it is picture-writing which must be translated into a form of thought corresponding to words before it brings a real picture before the mind's eye. This is symbolical art in which the artistic representation is a mere sign appealing to and stimulating the constructive imagination of the spectator to fashion an inner picture of his own making. It is not yet a work of real art which has its life and unity in itself, and attracts and holds the eye of the spectator at its most living point of interest.

This principle of composition was first carried out by the Greeks, when they left the sphere of symbolical picture-writing, and presented scenes with a real centre of interest and design. In the earliest works of Greek art, such as the Chest of Kypselos and the François vase, we have the oriental arrangement of tier upon tier of successive figures. It is in athletic vase-paintings like this black-figured archaic one that we have the first instances of composition. In the centre are the two boxers engaged, to the right and left are the Ephedros and the Paidotribes, facing the centre. By their

attitude as well as their action these two figures give a completeness to the scene, separate it from the outside, and drive the eye towards the real centre of action and interest. Unity, life, and variety are at once given to the whole scene. Unity in that the scene has a localised centre of interest towards which the other parts tend and lead; life in that each part contributes to the unity of the whole; variety in that there is a gradation of interest as we approach or leave the centre. In this simple and conventional form of work we have in embryo all the germs of the highest variety of composition. The attendant figures on either side represent the foreground and background to the central combatants, and we need but reach the perfection of technique in the acquisition of the laws of perspective, the power of shading, and the gradation of tones of colours, to carry this fundamental principle to its highest variety and expressive power.

It was in the palæstra that the early painter had the centre of artistic interest impressed upon him by the combatants whose struggle engrossed the attention of all spectators, it was here that he had this rudimentary form of composition impressed upon his eye by the ever-recurring figures of the Ephedros and the Paidotribes standing on either side.

Yet not only by *a priori* probability is this statement supported. The monuments themselves, if carefully studied, give the weightiest evidence. In the first place, the earliest works of art do not give us this form of composition, it comes in with the athletic vases. Furthermore, if we analyse the later vases, even those representing subjects most "unathletic" and of late complex forms, we can always trace this simple schematic form here given in the pugilists, the Ephedros and the Paidotribes. I have chosen these diagrams, serving to illustrate quite different lectures, at random. Here you have a scene representing the birth of Athene, here another relating to a tradition of Athene Ergane, and in all you have the two chief figures in the centre with standing figures on either side facing them. Sometimes the side figures are doubled, sometimes there is but one central figure in the middle, but the scheme remains the same. Here you have late vase-paintings with numerous figures, free and bold in composition and execution, representing an Amazonomachia and a Gigantomachia, and all this large group resolves itself into smaller groups of the form of this early athletic vase. However complicated and perfect the composition of a late vase, the traces of this simplest form of pictorial composition will always be noticed, the fundamental principle of pictorial art which was impressed upon the eye of the artist through the athletic games of the Greeks.

What we owe to the Greek artist constitutes the principle of art even in our time; it is the combination of nature and the ideal in the human figure, and the principle of composition in pictorial art, both of which were developed in him chiefly through the influence of the athletic games, and this fact I hope to have made clear to you this evening.

From the nature of the subject dealt with in this address we have necessarily only noticed Greek art in its expression of the physical side of human life, leaving unobserved the spiritual side of their great works. There is an erroneous notion abroad, started by those who have but a superficial acquaintance with Greek art, that though the Greeks represented with perfection the physical side of beauty, they failed to render due justice to the spirit and the soul. If sufficient time were at my disposal, I believe that I could show you how erroneous is this notion. It is true the Greeks avoided the expression of physical emotion in their statues when it led to grimace, yet their great statues are replete with the true soul of art. The soul of art does not depend upon the immediate expression of emotion in facial changes, any more than goodness with man depends upon the immediate act of charity in the most restricted sense. It may be a truer and greater act of charity to teach our pupils mathematics when pleasure calls us away, or to conform to the laws of good-breeding when our inclination and comfort drive us the other way, than to distribute a small share of our ready money to some beggar. The soul of art is not to be found in the immediate attempts at representing what we believe to be the outer manifestation of human souls; but in the unity and harmony of organisation given to a work through the design inherent in the creative artist's mind, the share of soul which the creating artist transfers from himself to the work of his hands, and above all in the complete and inseparable harmony that obtains between the subject represented and the material which embodies the idea. A marble angel of death bearing heavenwards in his arms a dead infant, with marble tears trickling down his cheek, suspended from the ceiling of a drawing-room by a silver cord, has less artistic soul than this Choiseul-Gouffier Pugilist; because in the athlete there is complete and inseparable harmony between the man represented and the artistic stuff that he is made of.

[C. W.]

WEEKLY EVENING MEETING,

Friday, April 20, 1883.

Sir Frederick Pollock, Bart. M.A. Vice-President, in the Chair.

Professor Bayley Balfour, Sc.D. M.B.

The Island of Socotra and its Recent Revelations.

The expression " our road to India " is one familiar to all inhabitants of Britain. It immediately brings to the mind's eye that long line of communication, commencing with Gibraltar on the west, and reaching through Malta, Cyprus, the Suez Canal, to Perim and Aden, by which intercourse with our vast Eastern Empire is maintained. But on that line of communication there lies in the Indian Ocean a large island, which, long before Britain had a road to India,—for India was not her possession—was the object of ambition to the rival nations struggling for supremacy in the East, but which in recent times, though sighted by all vessels passing by the Red Sea route to India or to regions east from Aden, has remained a veritable *terra incognita* on the threshold of civilisation.

This island of Socotra lies off the N.E. corner of Africa in lat. 12° 19′ to 12° 42′, and long. 53° 20′ to 54° 30′. Its extreme length from east to west is about 72 miles, and its breadth about 22 miles.

From Cape Guardafui 140 miles, it is a little more distant from the Arabian Coast (about 500 miles from Aden), and still further away from the Indian Peninsula.

It is the most easterly elevation of land on a coral bank lying to the N.E. of Africa, upon which, between it and Cape Guardafui, other islands (Abd-al-Kuri, Kal Farun, Sambeh and Darzi—known commonly as The Brothers—and Saboynea) of smaller size occur. On no part of this bank is the depth of water over 200 fathoms, but between it and the African coast is a channel reaching 500 fathoms. Around Socotra is a narrow coral reef.

Perhaps no island of like extent, and lying, as one may say, on the threshold of civilisation, has remained in later times so generally unknown as Socotra. Situated on the highway of traffic to the East by way of Suez and the Red Sea, it is almost invariably sighted by steamers making for or from the Gulf of Aden, and thus to those who nave passed along this route, its locality, or at least its name, will be known. To the scientific world it has been familiar as the country of a kind of aloes, the designation of which as Socotrine, has by some been traced to the name of the island. But to the majority of

people its existence and its name are alike unknown, or at most it is associated in a vague sort of way with the East Indies.

The causes for this are not difficult to discover. As the extreme outlying land in this region of the Indian Ocean, the island is exposed to the full blast of the monsoons, however they blow, and possessing no harbour in which a ship can at all times ride safely at anchor, it offers no inducements to ships seeking shelter. Then the currents which sweep past its shores run with considerable force into the Gulf of Aden, and there have been several shipwrecks on it, as well as on the African coast adjacent—the high hills of the island being easily mistaken for the mainland, and *vice versâ*—and navigation in its vicinity is altogether somewhat hazardous. It is not surprising, therefore, that passing vessels avoid the island as much as possible. Moreover the want of intercourse with the island, and consequent ignorance regarding its inhabitants, have given currency to various rumours not favourable to their character, which, though quite unwarranted, yet have had their influence in preserving Socotra as a virgin and unexplored island in the pathway of civilisation.

Its position on the direct route to India is one of far too much importance to have allowed its remaining so neglected had any natural advantages obtained permitting of its being utilised, or had there been no obstacles. Strategically valuable as is Aden, our station in this region, its barren waterless soil would place it at a great disadvantage compared with an island possessing a rich soil and plentiful water-supply, such as Socotra, did it possess the other elements necessary for becoming a military station. But it has been tried and found wanting. Its history shows how at various periods its importance has been recognised, and certainly its present backward condition can hardly be ascribed to want of attempts to settle or to colonise it.

The early history of Socotra is of considerable interest. The island seems to have been known to Europeans at an early period under the name of *Dioscoris* or *Dioscorida*,—a name traced by some to a Sanskrit root signifying "abode of bliss"; by others to two Arabic words meaning "island of dragon's-blood" (*kâtir* being the Arabic name for this gum-resin). This name was apparently applied at first, not to the one island we now know as Socotra, but to the whole archipelago of which it is a member. But possibly there is an old reference to the island under another name. The disputation that has taken place about the identification of the various spots from which in earliest times, as recorded in the Old Testament and on Eastern monuments, incense, myrrh, and other like substances were derived, is well known. The country of the Sabæans and of the Queen of Sheba is still far from being a settled question. Now on the Deir-el-Bahari monument at Thebes, erected by Queen Hatasou in the eighteenth dynasty, there are representations showing the commissioner of the queen going over the sea to the country of *Poun* and of *To Nuter*, and bringing back therefrom amongst other things

plants bearing *Ana*, which is shown as a gum-resin in the form of tears on the stems of small trees. The famous Ægyptologist Mariette has recently identified the land of Poun—Pliny's country of the Troglodytes—with Somali-land, the name being preserved in the modern *Bennah*, and the To Nuter of the inscriptions is, in his opinion, the Sacred Islands of Pliny, and the modern archipelago including Socotra. This identification would make the historical record of Socotra a very ancient one. How far the scientific evidence supports such identification is referred to subsequently.

The author of the 'Periplus of the Erythrean Sea' refers to it as a desolate island inhabited by a mixed population of Arabs, Indians, and Greeks, all speaking Greek, who had come thither in search of grain, and carried on a trade with the West Coast of India and with Mokha. The island is frequently mentioned by the early Arab geographers, who account for the Greek population by the story, which Colonel Yule considers a myth, that Alexander the Great, acting on the advice of Aristotle, settled an Ionian colony there, in order to cultivate the aloe. They further state that the Greeks and other inhabitants were converted to Christianity, and that clergy from Persia regularly visited the island. The population at this time, a few centuries after the Christian era, is put down by some at as much as 10,000, the majority of whom are described as Nestorian Christians and pirates.

In the time of Marco Polo, towards the end of the thirteenth century, the island was a metropolitan see of the Nestorian Church. Many ships visited the island, all vessels for Aden touching there, and the trade was mainly in ambergris, cotton stuffs, and salt fish. The people had the reputation of being enchanters, and of being able at will to raise the wind, to bring back ships, and to produce storms and disasters.

Although so mixed a population lived on the island, yet from the earliest times it appears to have been under the rule of the Mahra tribe, dwelling on the opposite coast of Arabia, whose sultan or sheikh lived at Keshin.

In 1503 Fernandez Pereira discovered it for the Portuguese, at which time an Arab sheikh lived in a fort at Zoko (modern Suk), then the capital of the island; but it was not until 1507 that Tristan da Cunha and Albuqerque captured the island for the Portuguese. After four years' occupancy the Portuguese retired from the island, leaving abundant traces of their presence. The remains of a fort on Hadibu plain, and at various places on the S. and S.W. sides of the island, are most substantial ruins. Besides in these, their influence is possibly seen in such names of places as Derafonta and in Feraigey, the name of one of the ruined forts, which may be Feringee. And indeed the dialect of Socotra may, it is thought by some, owe part of its peculiarity to a Portuguese basis. Moreover, at the present time, a large section of the inhabitants of the hill-region of the island

claim direct descent from the Portuguese. About this date the character of the Christianity had somewhat changed, and the doctrines of the Jacobite sect were professed.

The evacuation of the island by the Portuguese allowed a return of the Sultan of Keshin, and in his hands it has ever since remained, with the exception of a short occupancy on three several occasions by a foreign race—in 1538 by the Turks, in 1800 by the Wahabbees, and by the British from 1834 to 1839.

Although the ships of the East Indian Company frequently called at the island during the seventeenth century, some meeting with a friendly reception, others finding the reverse, and carried on a small trade in aloes and dragon's-blood (the stormy weather seems always to have been a source of dread), it was not until the year 1800 that affairs in the East directed the attention of the British Government to Socotra as a desirable possession, and the commander of the naval station in that region was directed to seize it. This was not done, and it was not until 1834 that the necessity for a coaling station induced the Indian Government to survey the island. This was accomplished by Captain Haines and Lieutenant Wellsted, and the result of the survey being satisfactory, the Government attempted to buy the island, but failing to do so it was seized in 1835 by Indian troops. Aden having been taken in 1839, and being more suitable as a coaling dépôt, Socotra was abandoned.

The exploration of the island by Wellsted supplied us with the first, and indeed until now only detailed account of the island, its people, and productions. The only available chart at present is the one made during this exploration, and it is most imperfect.

After the abandonment by the British in 1839 there is but scant record of Europeans visiting the island. In 1847 the French exploring brig, *Duconadic*, under Captain Guillain, and with the celebrated French collector Boivin on board, touched at the island for a few days; but except for an occasional shipwreck bringing it into notice one reads nothing about the island until 1876, when a prospect of its being occupied by another power caused the British Government to turn attention to Socotra, with the result that in that year a treaty was concluded with the Sultan, by which he binds himself, and his heirs and successors, "amongst other things, to protect any vessel, foreign or British, with the crew, passengers, and cargo, that may be wrecked on the island of Socotra or its dependencies, and he receives an annual stipend of 360 dollars for this." The "other things," it is understood, include a promise never to cede Socotra to a foreign power, or to allow a settlement on it without consent of the British Government. Thus the Sultan becomes a feudatory of Britain.

The attention of naturalists had long been directed to Socotra as a field for investigation whence rich results might be obtained, and Captain Hunter, who had visited the island in connection with the concluding of the treaty just mentioned, having brought back most

encouraging accounts, Dr. Sclater in 1878 brought the matter prominently before the British Association for the Advancement of Science, and the Committee of the Government Fund for Scientific Research, with the result that a certain sum of money was obtained, and a committee appointed to take steps for the exploration of the Natural History of the island.

Various causes delayed the sending out of the expedition, and it was not until January 1880, that I left this country, returning again in April, having spent, with two companions—Lieutenant Cockburn, 6th Royals, whose regiment was at Aden, and Alexander Scott, a gardener from the Royal Botanic Garden, Edinburgh, who accompanied me from England — nearly seven weeks on the island. Although so long a period elapsed between the evacuation of the island by the British and the date of our expedition, yet we were followed in the succeeding year by the German traveller and botanist Dr. Schweinfurth, who with three companions—MM. Riebeck, Rosset, and Mantay—spent five weeks on Socotra. Thus, after an interval of forty years, the island has been explored in two successive years.

Such is a brief historical account of the island up to the present time. And I may now give a short account of the government and people.

The government of the island is in the hands of the Sultan of Keshin and Socotra. At present two brothers are joint Sultans, and one lives at Keshin, the other resides in Socotra. They are nephews of the one who, in 1834, refused to sell the island to the British. The Sultan has complete sway in Socotra. He has a residence on Gharriah plain, at the base of the Haggier hills, and has also a palace in Tamarida, where he dispenses justice. Under him, each of the large villages has its sheikh or head, and the island is divided into four sections, each of which is in charge of a ranger. The Sultan alone has power to inflict punishment. In each section the land is let out to the various tribes of Bedouins, both for pasture and for the collection of gum, payment therefore being made in ghi. The Sultan reserves for himself one portion of land for the collection of dragon's-blood.

The trade of the island at present is small, ghi being the chief export. It is carried on by buggalows from the Arabian coast. "These arrive in the first months of the year with coffee, rice, and other articles, which they exchange for ghi, aloes, orchella, weed, &c., which they take to Zanzibar, and, on their return, they bring coconut, bombé, and American piece-goods. They dispose of as many of these as possible, and take outwards ghi, aloes, dragon's-blood, blankets, &c., and return to Arabia. Pearl-fishers from the Persian Gulf at times visit the island and dispose of their pearls. The Sultan takes tithe of all exports. From ghi his revenue is about 500$, aloes bring him 250$, edah gives 80$, and other sources bring it up to 1000$ a year, which, with his stipend of 360$ from the British, makes him a comparatively rich man in this region."

The extent of the population it is impossible to estimate, as so many people live in caves, and one only occasionally comes across the wandering inhabitants of the hill region. The number has been set down as low as 4000 and as high as 10,000.

In speaking of the people, the dwellers on the shore must be distinguished from those on the hills. The former, who are a mixed population of Arabs, Indians, and Africans of various tribes, live in small towns. Of these the chief one is Tamarida, on the extensive Hadibu plain at the base of the Haggier range of hills. It is the capital of the island, and consists of a number of stone and lime houses, of the ordinary construction seen in Arabia, all plastered outside of a dazzling white, and surrounding a large one, which is the Sultan's palace. Around the town is a dense date-grove. There is a mosque and well-filled graveyard in the centre of the town. The number of inhabitants is set down at about 400. Kadhab is another village, lying on a sandy spit east from Tamarida. The houses here are of the same character as at Tamarida, and there is a mosque. Gollonsir, at the west end, is a penal settlement, and has but few houses. Formerly, the capital of the island was Suk, at the east edge of the Hadibu plain, but it was destroyed. There are numerous small villages all along the coast line, but the three mentioned are the chief towns.

The occupation of the residents in these villages is mainly fishing. They cultivate small tracts of ground near their houses, but are, as a rule, idle. The population too is somewhat changing, many going off in trading buggalows to Zanzibar or the Arabian coast.

The inhabitants of the hills, "Bedouins," as they are called, are very different people. They are regarded as the aborigines of the island, and alone possess any great interest ethnologically. They are mostly troglodytes, but here and there live in small huts, with stone and lime walls and roofed with date-palm leaves. They are a most peaceable race of people, and are divided into numerous families belonging to a few principal tribes. A study of these tribes would well repay the time and trouble spent upon it. Captain Hunter says: "The 'Karshin,' who inhabit the western end of the island, claim to be descendants from the Portuguese. The 'Momi,' who reside in the eastern end of the island, are said to trace descent from the aborigines and the Abyssinians; whilst the 'Camahane,' who live in Haggier and the hills above the Hadibu plain, claim to arise from the intermarriage of the aborigines with the Mahri Arabs from the opposite coast. Whatever be their origin, certain is it that the hill people have a very distinct appearance. Many of them are tall and finely made, the men with broad shoulders, lean flanks, and stout legs, reminding one very forcibly of the European build. Thin-lipped and straight-featured, they have straight black hair. The women are many of them very good-looking, somewhat resembling gipsies, but they have rather large hands and feet."

The men wear a loin-cloth, one end of which is commonly thrown

over the shoulder, usually with a knife stuck in the waist, and they invariably carry a stick. The women have the ordinary Arab blue skirt, in most cases kilted at the knees and continued round the waist by a girdle. In some cases, however, they improvise a petticoat of the coarse blankets they themselves weave, and wear on the upper part of the body a loose tunic with short sleeves. They go unveiled. The women wear the hair done up in two plaits which hang down their back, but in front the hair is cut to form a short fringe on the forehead. Their ornaments are few. The men often wear an armlet of silver. The women have necklets of amber, glass beads, dragon's-blood tears, or in some cases rupees, and have also the ordinary Arab silver armlet and ear-rings.

The occupation of these people is chiefly pastoral. Their herds and flocks are extensive. From the milk they make quantities of ghi by a simple process of churning—merely continuous jerking of the skin mussocks—and they sell it to the Arabs of the coast, or exchange it for rice, dates, or other necessaries. They collect also dragon's-blood and aloes, but the latter only in great amount when pasturage fails them. The women spin a coarse thread from the sheep's wool, which they weave into blankets.

Old voyagers speak of horses being used, but there are none now. The cattle are small and have no hump. Immense herds are found at the eastern end of the island. The sheep are all fleeced, but there are none of the Berbera kind. Of goats there are some in a wild condition. The camels are much smaller than those at Aden and elsewhere in Arabia, and are able to climb like goats; many are kept for milking. Asses roam wild in herds all over the island.

Of plants cultivated on the island the most important is the date-palm. Every stream on the island is lined by groves of them, and the fruit is used, both ripe and unripe. Melons are grown, as also small onions. Little cereal culture is indulged in. Here and there, on the hills beside a stream, a small enclosure of "bombé" (jowari) may be seen, but the inhabitants are too lazy to cultivate to any extent, the watering requiring too much labour. Only in one spot was there observed an attempt at irrigation.

These hill people live very miserably. Milk forms a large portion of their diet. Bombé is used when grown. Rice is obtained from the coast Arabs. Date is a staple of food. On great occasions a sheep or a kid is killed.

The furnishing of their dwellings is very meagre. Blankets are their couches. Goat-skin mussocks are used for water and milk. They have also earthenware pots, moulded by the hand out of the clays and lime of adjacent rocks.

Their language is peculiar. Captain Hunter says of it: "I could trace no affinity to any of the languages of the neighbouring coasts. It sounds a little like Kis Swahili, but not so soft. It is not Mahri, for the Sultan said it in no way resembled it. The sound is not so guttural as Arabic, and seems to require less effort in enunciation."

Somalis do not understand it. Wellsted says the people of the opposite Arabian coast understand it, but that is not the case. Perhaps Portuguese may have had something to do with it. Others trace in it a Phœnician basis.

"Religion sits lightly on a Bedouin. All are Mussulmen, but they only pray when they have an audience, and even in the very act of prostration they will turn round and join in the conversation, and again continue their devotions until the requisite outward observances have been completed."

The fact that the Wahabbees visited the island accounts probably for the absence of the many churches, or traces of them, said to exist in ancient times on the island. Wellsted observed some ruins, believed to be of a church. There are, however, still evident the ruined forts of the Portuguese. The largest of these is at Feraigey. No written records have been found; possibly such would disappear along with the churches. Wellsted speaks of inscriptions on the rocks being visible. None of these were seen by us. But on the Kadhab plain there occurs a broad pavement of limestone, 50 yards long by 25 to 30 yards broad, whereon numerous hieroglyphics are cut. The figures are not in line, and do not give the idea of any continuous sentence, and they lie at all angles to one another and at varying distances. Some resemble foot-imprints, others distinctly represent a camel, others are like St. Andrew's cross, others are of most irregular form.

The physical features of the island may now be noticed.

The surface features of Socotra at the present time are those of an island mountainous in the extreme. The shore line on its southern aspect is, as the map shows, a tolerably continuous one, unbroken by deep inlets or bays. On the northern side occur a few shallow bays at the mouths of the streams, which afford the only anchorage to be obtained around the island, but no one of them is safe at all seasons of the year. On all sides the hills rise with considerable abruptness over a wide area, forming bold perpendicular cliffs of several hundred feet in height, whose base is washed by the waters of the Indian Ocean; but at other places leaving plains varying in breadth up to as much as five miles between their base and the shore. On the south side of the island is the largest of these shore plains—Nogad—which, extending nearly the whole length of the island, is for miles covered with dunes of blown sand. On the north these plains occur chiefly at the mouths of the streams, and are the sites of the only places which may be called towns.

The internal hilly part of the island may be roughly and shortly described as a wide undulating and intersected limestone plateau of an altitude averaging 1000 feet, which flanks on the west, south, and east a nucleus of granitic peaks over 4000 feet high. The whole of this hilly region is deeply cut into by ravines and valleys. These in the rainy season are occupied by roaring torrents, but the majority of them remain empty during the dry season. There are,

however, many perennial streams on the island, especially in the central granitic region, where amongst the hills the most charming bubbling burns dashing over boulders in a series of cascades, or purling gently over a pebbly shingle, make it hard to believe that one is in such proximity to the desert region of Arabia. Few of the perennial streams reach the shore in the dry season—most of them are fiumaras.

The eastern end of the island is most destitute of water. Here in the dry season are no rivers, and, springs being rare, it is the most arid region.

The geology of the island is not very complex. The fundamental rocks are gneisses, both hornblendic and granitoid, belonging, like those of north-west Scotland and of north-east America, to the earliest Archæan age. These crop out on the hill slopes and in the valleys, but do not as a rule form the exposed higher parts of the island. Through this fundamental mass cut felspathic granites of varying texture, and containing little besides quartz and felspar, which form the central nucleus of fantastic peaks, the highest part of the island. Cutting through both the forementioned series we have other granitic rocks, such as minette, felsite, rhyolite, and also basalt and diorites, in many places forming large dykes, and in others extensive lava flows. The centres of ejection of these rocks it is impossible now to determine, and possibly many of them, as in the case of the Tertiary volcanic rocks of East Hindostan, have been discharged, not from cones, but as outflows from fissures. Towards the south-east end of the island we find them in greatest abundance, and exhibiting a very fluidal character. The date of the eruption of these rocks was certainly pre-miocene. An indurated shale (argillite) is found in some localities, notably on Hadibu plain, and with it a little sandstone of uncertain date, but probably representing the well-known Nubian sandstone of carboniferous age. Over all comes a capping of limestone, forming plateaux over wide areas, rising in abrupt cliffs two or three hundred feet high. It is generally of a yellowish or whitish colour, compact, and sometimes slightly dolomitised. It contains numerous foraminifera, which prove it to be probably of Middle Tertiary age, or rather later than that of Sinai and the Arabian shores of the Red Sea. The surface of the limestone over extensive districts is rotted and broken into a jagged surface, over which progression is by no means easy, whilst at other spots it forms broad smooth slabs. Subsequent to the laying down of the limestone there occurred further volcanic disturbance, and the limestone is cut through by dykes of basalt and compact trachyte of late Tertiary age.

The soil resulting from such petrological conditions is correspondingly varied, correlated with which is a varying character in vegetation and scenery.

In the valleys on the banks of the stream, especially in the granitic region, a deep rich red soil is found, and where there is water perennially it is covered by a luxuriant growth. As the limestone

composes the greater part of its superficies the plateau appears barren. Where, however, the limestone has rotted, a series of nooks and crevices occur, in which, where a soil has collected, an *Aloe*, *Kalanchoe*, or other succulent finds a congenial habitat. But upon the limestone plateau, especially at the eastern and western ends of the island, occur depressions varying in width from some hundred yards up to a mile or more, girt on every side by a cavernous limestone cliff, with perhaps a narrow outlet through it at one or more points. These, which have all the appearance of lagoons, or at least of enclosed water-basins, are floored now by a rich red soil on which a crop of coarse grass, small herbs, and low trees vegetates. On the shore plains the soil is light and sandy.

In its climate Socotra contrasts favourably with the adjacent shores of Arabia and Africa.

During the N.E. monsoon, from October to April, it is cool. January and February are the most pleasant months. But during the rest of the year it is exceedingly disagreeable. Rain falls twice in the year, at the changes of the monsoons, at which time the stream-courses are filled with mighty torrents. The temperature of course varies much with the altitude, and one may pass in the course of a few hours from the tropical heat of the shore plains to the cool temperate air of the mountain ranges. The average temperature on the plains in January is said to be about 70°, but in the hotter months is as much as 86°. But on the plateaux the temperature at nights often goes down to 52°. The higher peaks are, at least in the cold season, frequently enshrouded in mists, and at night very heavy dews fall. The climate on the hills is very healthy; but on the plains, especially at the changes of monsoons, fever is prevalent.

Of zoological features one of the most striking is the paucity of indigenous mammals. The antelopes and rodents of the adjacent continents are absent from Socotra, and there are but two mammals indigenous: a bat—of which, unfortunately, we did not obtain a specimen—and a civet cat. This latter is a type widely dispersed in South Asia and tropical Africa. Rats and mice occur in the villages, but are probably introduced. Birds are plentiful, so are lizards, and there are some snakes. The rivers are stocked with fish, and in them crabs are also found in abundance. Land mollusca are, as might be expected, frequent, and the whole island teems with insect life.

Considerable interest attached to an investigation of the avifauna of Socotra. It is well known that in several Indian Ocean islands large so-called wingless birds formerly existed, several of which have become extinct within recent historical time. The Epiornis of Madagascar, the Dodo of Mauritius, the Solitaire of Rodriguez are examples. Vague rumours credited Socotra with the possession of a ratidine bird of like character; Wellsted in his account of the island speaks of it as a Cassowari. Of such a bird no traces exist at present, nor could any legendary reference to such a bird be discovered.

As at present known, the avifauna includes forty-three species. On the shores we find gulls and herons, on the streams wild-duck and plovers, the date-groves are tenanted by doves and pigeons, whilst all over the island weaver-birds, chats, shrikes, sunbirds, and sparrows abound. Cuckoos and falcons are occasionally met with, whilst in the vicinity of habitations the scavenger-hawk of the East and a carrion crow are ready to perform their offices. A few quail occur on the plains. All the birds except the Passeres, Picarieæ, and Columbæ, are of wide distribution. The Passeres are the most numerous of all, and include eight species not known from other regions, and two of these belong to a new type of sparrow—*Rhynchostruthus*—characterised by the massive form of its bill. The sunbird, as might be expected, is new, and is of interest from having no metallic colouring on its plumage. A small lark on the plains has a peculiar plaintive note, but the song-bird of the island is a new starling, its melody equalling that of a thrush.

The snakes of the island are very peculiar. Of the four species known three are endemic, and two of them are so distinct as to form new genera with Asiatic and Mediterranean alliances rather than African, the fourth is the same as one brought by Tristram from the shores of the Dead Sea.

Great interest always attaches to the land and fresh-water mollusca of a large and ancient island, and in this feature Socotra is not disappointing; for a very large proportion of the shells are endemic, and of the genera to which they belong some have a very instructive distribution. Thus *Otopoma* is restricted to the East African islands and Arabia, *Lithidion* has the same area but extends to India, *Cyclotopsis* is represented outside Socotra only in India and the Seychelles, whilst *Tropidophora* is known from Madagascar alone.

In all other animal groups interesting alliances of similar nature are discoverable.

On account of our scanty knowledge of the fauna of Socotra and our still slighter acquaintance with that of the adjacent mainland, it is impossible to estimate the proportion of the endemic part of the fauna. But what we do know shows very clearly the strong African facies and relation to the faunas of the other African (East) islands, and at the same time indicates the occurrence in great force of Arabian and S.W. Asiatic forms as well as a clear strain of Indian and Eastern resemblances.

The vegetation of the island varies in aspect with the character of the rocks. Starting from the shore one finds no representative of a marine phænogamic vegetation, although in the stagnant brackish waters at the mouths of the streams naiads occur. The coast is not favourable for seaweeds, being too shingly and sandy.

On the dry sandy plains the vegetation typical of the desert regions on the mainland reigns; small-leaved, stunted, and woody bushes and herbs, often so rigid as to become spiny, or fleshy plants without foliage-leaves prevail. Aromatic odours are a marked

feature of many plants, and also the occurrence of gums and resins, which in some cases appear as natural exudations in the form of tears. The common desert characteristics of a glaucous grey coloration or a hairy pubescence mark also many of the plants. The flora of this region is indeed thoroughly that of the Arabo-Saharan type, such genera being abundant, as *Fagonia*, *Cleome*, *Ærua*, *Farsetia*, *Balsamodendron*, *Anticharis*, *Breweria*, &c.

Leaving the plains, and passing to the hill slopes and valleys, plant life is more vigorous, but in no place sufficiently so to call for the designation of forest, nor is there anything in the way of fine timber. But in the valleys, wherever there is any degree of moisture, small trees of some 20 to 25 feet, with smaller shrubs packed so densely as to exclude the light from above, linked together by far-reaching lianes, and underlain by a thick under-scrub of fern and herb, make an almost impenetrable thicket, and produce a verdure quite tropical in its luxuriance. In this district the type of flora is of the general tropical old-world type, having representatives of such genera as *Grewia*, *Ormocarpum*, *Dichrostachys*, *Dirichletia*, *Lasiosiphon*, &c.

Once out of the valleys and upon the plateaux the scene is essentially different. Wide barren stretches of grey limestone extend on every side unrelieved, save by an isolated Dracæna, or tree-Euphorbia of stiff erect habit, looking like the remnant of the vegetation of some old geological epoch; or where a lake-like depression, with its brown earth sparingly coated with green herbage, intervenes. And again, reaching the higher altitudes on the granitic range, the vegetation impresses one at once with its sub-temperate character. The arborescent type has almost entirely disappeared. Shrubby composites such as species of *Psiadia*, *Pluchea*, and *Kleinia* are found, and quaint types such as those of *Thamnosma Nirarathamnos* (*Umbelliferæ*), *Cephalocroton*, *Dorstenia*, *Adenium*, &c., are frequent. Twiggy, narrow-leaved herbs form a dense deep carpet on the soil, interrupted here and there by a protruding lichen-covered boulder, and for all the world like the covering of heather on a Scottish moor; whilst within the shade of the boulders, or in the moisture of the overhanging cliffs in the ravines, bright green herbs, such as species of *Galium* and *Gypsophila*, nestle in beds of liverwort and moss, so that it would require no very great effort to believe one was exploring an Alpine crag in a temperate region.

The flora is a pretty extensive one. It comprises, as we know it, somewhere about 600 species and varieties of Phænogams, 20 species of Vascular Cryptogams, whilst Cellular Cryptogams number about 300 species. The extent of the flora is thus 900 to 1000 species.

Amongst Phænogams the proportion of Monocotyledons to Dicotyledons is as 1 to 6, a proportion somewhat smaller than is usual in a tropical island flora, but this is due possibly, not to the absence of the former, but because collectors have usually visited the island when they are not showing above ground.

Of the families most numerously represented, *Leguminosæ* and *Gramineæ* each embrace about 1-11th of the whole Phænogams, and they are closely followed by *Compositæ* (about 1-14th), *Acanthaceæ* and *Euphorbiaceæ* (each about 1-20th), and *Cyperaceæ* (about 1-25th). *Orchideæ* are represented by but one species. *Lichenes* are most numerous of Cellular Cryptogams.

The flora of a continental island such as Socotra is in the main interesting in connection with the geographical distribution of plants and the working out of the history of their migrations over the face of the globe. But in the flora now under discussion there are a number of special features in individual plants well deserving of attention, and I may now notice some of them.

Of plants striking as having brilliant flowers may be noted the *Adenium*, from which Aden is said to derive its name; a tuberous *Begonia*, which has been introduced into horticulture; a fragrant *Crinum*, species of *Exacum*, *Ruellia*, *Jasminum*, &c. Few wild plants yield edible fruits. The jujube is alone abundant.

On morphological grounds there falls to be noticed firstly *Dendrosicyos socotrana*, known to the inhabitants as the *camhane* tree, a new genus of Cucurbitaceæ. This plant differs from the ordinal characters in being a tree with a stem often four or five feet in diameter at the base, rapidly tapering, and forming a very soft juicy wood. Another plant of interest, on morphological grounds, is a small tree bearing a fruit like a pomegranate, but instead of having the double row of carpels characteristic of the true *Punica granatum*, there is but a single whorl. Can this be the primitive type of the Pomegranate? Another morphologically interesting plant is a Menisperm, a *Cocculus*, which differs from the ordinal type in being a hard erect undershrub, with cladodes and short spiny branches.

Of plants interesting for their products we have several in Socotra. Amongst them the first place is claimed by the Dragon's-blood tree, *Dracæna Cinnabari*. The dragon's-blood of commerce at the present time is, as is well known, the product of *Calamus Draco* of Sumatra. But the Socotran gum-resin is the old κιννάβαρι mentioned by Dioscorides. It is known on the island as *edah;* amongst the Arabs it is *kâtir*. The plant is endemic, and nearly allied to the *D. Draco* of Teneriffe. From the other gum-resin-producing species, *D. Ombet* of Abyssinia and *D. schizantha* of Somali-land, of which we have as yet but imperfect knowledge, it is apparently quite distinct. The gum-resin exudes in tears from the stem of the tree, and is collected after the rains; the gatherer chipping off the tears into goat-skins. There are three forms in which the gum-resin is exported. Of these *edah amsello*—the tears as they exude from the tree—is the purest and most valuable form; $2\frac{1}{2}$ lbs. fetch one dollar. The second-best kind is called *edah dukkah*. It consists of the small chips and fragments of the tears which have been broken off in separating the gum-tears from the tree, or by attrition; it sells at one dollar for 4 lbs. The cheapest is the *edah mukdehah*, which brings a dollar for 5 lbs.,

and is very impure. It is in the form of small flat-sided masses, and consists of fragments of gum-resin and refuse of the gatherings melted together into a flat cake, and then broken up into smaller portions.

Of other gum-resin-producing trees on the island, the frankincense and myrrh trees must be noticed. I have already referred to the discussion that has taken place regarding the incense country of the ancients. The Hadramaut country is the incense region *par excellence*, and to its kings Socotra is said to have been subject. But Socotra is identified on ethnological grounds by Mariette as the "To Nuter" of the Theban monuments, the "Sacred Island" of Pliny, and the "Isles of the Sea" of the Old Testament. Now we find the genus *Boswellia*, which yields frankincense (olibanum), represented in Socotra by no less than three species, all of which are endemic, and possibly there is a fourth, and as there are only three other known species of the genus, all of which save one are Somali-land plants, the proportion occurring in Socotra is very large. The commonest frankincense in the island is the *ameero*, but it is not much exported.

Of myrrh plants Socotra possesses no less a share. Besides the *Balsamodendron Mukul* which yields the "Indian bdellium"—the *googul* or *mukul* of the Arabians,—there are probably five other species of the genus on the island. Possibly one of these is the Arabian *B. opobalsamum*, the true myrrh plant. The myrrh collected is termed *leggehen*, and is said to be exported.

So far then as the occurrence of frankincense and myrrh producing trees is evidence, Socotra may well be the To Nuter of Theban monuments; for probably no area of equal extent has so many peculiar forms.

Probably the most important plant of the island, so far as products are concerned, is the *Aloe Perryi* which yields the "Socotrine aloes" of commerce. The gum is known as *tâyef* by the natives; the Arabs call it *sobr*. Although this kind of aloes has been so long known, and has the reputation of being finer than either Barbadoes or Cape aloes, it is only within the past few years that the character of the plant has been made known. It grows abundantly on the island, especially on the limestone plateaux. The collection of the gum is a very simple process, and can be accomplished at any season. The collector scrapes a slight hollow on the surface of the ground in the vicinity of an aloe-plant, into which he depresses the centre of a small portion of goat-skin spread over the ground. The leaves of the aloe are then cut and laid in a circle on the skin, with the cut ends projecting over the central hollow. Two or three layers are arranged. The juice, which is of a pale amber colour with a sweet, slightly mawkish odour and taste, flows from the leaves into the goat-skin. After about three hours the leaves are exhausted; the skin is removed from beneath them, and the contained juice transferred to a mussock. Only the older leaves are used. The juice thus collected is of a thin watery character, and is known as

tâyef rhiho, or watery aloes. In this condition it is exported to Muscat and Arabia, and sells for three dollars the skin of 30 lbs. By keeping, however, the aloes changes in character. After a month the juice gets, by loss of water, denser and more viscid; it is then known as *tâyef gesheeshah*, and is more valuable—a skin of 30 lbs. fetching five dollars; whilst in about fifteen days more—that is, about six weeks after collection—it gets into a tolerably hard solid mass, and is then *tâyef kasahul*, and is worth seven dollars a skin of 30 lbs. In this last condition it is commonly exported.

There is, as I have said, no forest on the island, and yet there is one small tree, or large shrub, which may be of some value commercially. It is the *metayne*, a kind of box-tree, *Buxus Hildebrandti*. It was first found by Hildebrandt on the Somali-land hills. It forms a hard, compact wood, and, I doubt not, might be used for many of the purposes for which boxwood is so valuable at the present time. It is abundant on the island, and Hildebrandt reported it very common in Somali-land. I did not bring home sufficient specimens to allow of an experimental trial of this as a material for woodcuts or other purposes. I learn from Dr. Schweinfurth, that he has sent some to Berlin to be tried in this way. Should the wood prove serviceable, it requires no special mention to indicate how valuable this product may become, in view of the exhaustion of the boxwood forests (of which we hear so much) in the S.E. of Europe.

Many plants are used on the island for the purposes of dyeing. But of these the only one that need be here referred to is the orchella weed (*Roccella tinctoria*). Occurring in abundance, it was formerly exported in great quantity. It is known as *shennah*.

Surveying the flora from the point of view of its relations and development, we shall consider the Phænogams alone. The limitations of genera and species amongst Cellular Cryptogams—the Thallophytes in particular—are so uncertain that they afford no basis for comparisons. And I must also state that in making statistical estimates of the relations of the flora, one can only do so in the most general way, as our knowledge of the flora itself is not complete, and then we know so very little of that of the adjacent mainlands.

Of the 600 species and varieties of Phænogams, which are comprised in 81 natural orders, and in 324 genera, about 200 are endemic, i. e. endemic species are to non-endemic ones in the proportion of about 1 to 3. This proportion is greater than is found in the Seychelles flora, and in that of the Mascarene Islands, but it is less than in the Madagascar flora, wherein Mr. Baker makes the proportion nearly 1 to 2. The endemic species and varieties are referable to 143 genera, and of these a proportion to the whole genera of the flora of about 1 to 16 is endemic. This is large compared with some other Indian Ocean Islands, but falls far short of Madagascar, where it is 1 to 7.

The species which are not endemic are almost all referable to genera of considerable dispersion, and of the species themselves we find that about 1-4th are cosmopolitan in the Tropics, many of them

weeds of cultivation. A large proportion, about 1-3rd, are species belonging to Tropical Africa and Tropical Asia. The greater number of these are limited to the plain region of North-East Africa, and South-West Asia, reaching as far as Scindh and Afghanistan, and there forming part of the great desert flora. But a few extend into the Indian Peninsula itself, even reach the Malay Archipelago, or spread into Australia (the latter form about 1-13th of the whole third). Westwards we find an extension of a considerable proportion of this third (about 1-8th), passing to the Mediterranean region and the Canary Islands, whilst some (about 1-6th) reach the Cape de Verde Islands. Of the remainder of the non-endemic flora, a fair proportion (about 1-16th) are found in Tropical Africa alone, chiefly in Abyssinia, and a smaller proportion (about 1-20th) are natives of Asia, chiefly Arabia, but are not African. A few are restricted to South Africa, a few are Indian alone, one is Mascarene and Indian, and one is Gorgonian.

In the endemic flora we find many plants of very antiquated types. These occur especially on the hill regions of Haghier. Such are *Dracæna*, tree *Euphorbia*, *Euryops*, *Aloe*, *Thamnosma*, *Cocculus*, *Dendrosicyos*, &c. We find also a fair number of genera whose maximum development takes place in temperate regions. Of the genera comprising this flora about 2-5ths have a wide tropical range, a fair number are common to Tropical Africa and Asia, and a few are chiefly Mediterranean. Amongst those having restricted distribution may be noted three, *Diceratella*, *Taverniera* and *Anisotes*, which are small genera of the plains of South-West Asia. *Campylanthus*, represented at Aden, has a single species in the Cape de Verde Islands. Three genera are essentially Tropical African—*Cephalocroton*, *Eureiandra*, and *Camptoloma*, and of these the last two are ditypic and found only in Angola. *Graderia* and *Babiana* are genera not found beyond the limits of South Africa; whilst *Lasiocarys* is a South African genus having a single representative in Abyssinia, and *Euryops* is a South African one, with one species in Arabia.

The only purely Indian genus is the ditypic *Priotropis*.

The occurrence in Indian Ocean islands of restricted New World types or of forms related to these is a remarkable and well-known fact of distribution. We have for example the Sapotaceous *Labourdonnaisia* represented in Natal, Mascarene Islands, and Cuba; the Laurineous *Ocotea* with representatives in Canary Islands, South Africa, and Madagascar, its main distribution being American; and the Rodriguesian *Mathurina* with its nearest ally the Central American *Erblichia*. In Socotra we have illustrations of the same peculiarity. The Rutaceous genus *Thamnosma* is otherwise represented in California by one species and in Texas by another, and its occurrence is the more remarkable as with the exception of *Peganum* it is the only genus of true Rues found in the New World. Amongst the endemic genera we have a like feature, for the Verbenaceous *Cœlocarpum* is almost congeneric with the tropical and sub-tropical American

Citharexylon. And the Geraniaceous *Dirachma* has its nearest affinities in the *Wendtieæ* and *Vivianieæ* of Chili and Peru.

To summarise the features presented by the Phænogamic flora of the Island of Socotra, we may say :—

1. It is that of a continental island and presents features of great antiquity.

2. Relative proportion of orders to genera and of these to species is large.

3. There are few annuals.

4. It possesses much individuality and further exhibits three distinct elements, (*a*) of a dry parched region, (*b*) of a moister tropical region, (*c*) of a cooler and more temperate region.

5. Its affinities are essentially Tropical African and Asian, but the African element predominates, and in the African element we find in great force the features of the flora of the mountainous region of Abyssinia, West Tropical Africa and South Africa, and also of Madagascar. This element of the flora too is that of the higher regions of the island (*c* of the last paragraph).

6. The flora of the dry region is the typical Arabo-Saharan.

7. The flora of the moister tropical region is that of the Old World tropics generally.

8. There are a few Indian and American types.

It may not be out of place to conclude these introductory remarks with a reference to what may be learned from the biological and physical features of the past history of the island and the changes it has undergone, and the way in which the features as we now see them have come to be.

The position of the island would *a priori* lead one to expect that it had formerly been a part of Africa. This it undoubtedly has been, but its separation from the mainland is of some antiquity. It is indeed, with Madagascar, to be regarded as the remains of a greatly advanced African coast-line at a remote period. This connection with the mainland explains the general African affinities of its fauna and flora; but there remains the problem of the South and West African relations for elucidation. The botanical features of Africa have long excited investigation and speculation. The Cape flora, as is well known, is one of a very old and highly specialised type, and in Northern Africa and Western Europe there are many plants in the flora which, as Mr. Bentham has pointed out, are very nearly allied to corresponding Cape species. Again, on the east side of Africa one finds on the Abyssinian hills and the range stretching south, a vegetation distinctly South African. Further, on the Cameroon Mountains and Fernando Po there are not many characteristic Cape types, but what there are, are identical with species of the Abyssinian hills. The relations and similarities of the floras of these different regions would all seem to point to the conclusion first promulgated by Sir Joseph Hooker, that the South African flora has been continued along the highlands of East Africa from

Natal to Abyssinia, and in like manner that there was a connection between the western region about Biafra and the Abyssinian district. It would appear probable that at a time when the tropical zone was much cooler than it now is, the northern forms of plant life spread over South Africa, but with the diminution of the cold of the glacial epoch they were driven back and retreated northwards, a few types left on the higher regions being the only evidence of the invasion and the survivors of the hordes extinguished. Thus at the present day on the higher lands of Abyssinia, West Africa, and in South Africa, we have the fragmentary traces of the extension of an old African flora, and of this flora Socotra would appear to exhibit on its hills the most north-easterly limit, just as Madagascar and perhaps the Mascarene Islands show the most easterly extension.

The African affinities being so explained, what of the Indian and Asiatic? The higher level of the land necessary to unite Socotra and Madagascar with the African continent, if continued over a slightly wider area, would produce some interesting changes in surface features. Africa would be joined to Arabia, the Persian Gulf would cease to exist, and the Tigris and Euphrates, united in one stream, would pour their waters through a delta occupying much of the Arabian Sea, and through which also the Indus would debouch. Thus a means of transit for the migration of Indo-Malayan types would be afforded. That some such connection as this did formerly exist all evidence conclusively shows, and that by this route migration took place is equally certain, and we have thus an explanation of Indo-Malayan affinities in Socotra and in Africa, without calling in the aid of the now untenable hypothesis of an "Indo-Oceania" or "Lemuria," making a complete land surface over what is now the Indian Ocean. Whatever be the date of the variations in level which brought about the present surface features in this region, it is clear that the separation of Africa from Asia and the formation of the straits of Bab-el-Mandeb took place before the final isolation of Socotra.

Australian affinities are explained on the same lines as are the Asiatic; but when we come to deal with the American we touch a matter of which at present we can give no adequate explanation. That the many forms identical generically in the Indian Ocean Islands and in America are sprung from one stock of great antiquity must be admitted, but whether the migrations which have led to their occurrence now in such antipodean regions were directed in an easterly or in a westerly direction, is one of those problems of which we have not yet the materials for a solution.

How long, then, has Socotra been an island? It is difficult, nay impossible, to picture the complete history of the island from the earliest geological epochs; but in brief some, such as this, may be sketched. During the Carboniferous epoch there was in the region of Socotra a shallow sea, in which was deposited on the top of the fundamental gneisses of this spot—which had ere now been certainly

much seamed and fractured by volcanic outbursts—the sandstone of which we have such a large development in Nubia. This sea subsequently deepened, allowing the formation of the shales, which now constitute the argillite of the island. During the Permian, Socotra may have been a land surface, forming part of the great mass of land which probably existed in the region at that epoch. In early and middle Tertiary times, when the Indian peninsula was an island, and the sea which stretched into Europe washed the base of the Himalayan hills, Socotra was probably under water, and the great mass of its limestone was deposited; but it is quite possible that at this epoch its higher peaks were still above water. Thereafter it gradually rose, undergoing violent volcanic disturbance, and again possibly became part of the mainland, though it is likely for only a short period, and subsequently it reverted to its insular condition, in which state it has remained.

An island, then, from Tertiary times, the various denuding agents have during that time sculptured its surface in the fantastic manner we see at the present day; and the fauna and flora have lost many of the old types which linked them with their primitive stock, retaining, however, some few as records of their origination, and at the same time have developed new and replacing forms, which are in their turn undergoing modification.

[B. B.]

WEEKLY EVENING MEETING,

Friday, April 27, 1883.

GEORGE BUSK, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Sir WILLIAM SIEMENS, D.C.L. LL.D. F.R.S. *M.R.I.*

Some of the Questions involved in Solar Physics.

THE lecturer introduced his subject by drawing attention to the circumstance that the idea of the sun being an exceedingly hot body was of very modern date; that both ancient and modern writers up to the early portion of the present century attributed to him a glorious and supernatural faculty of endowing us with light and heat of the degree necessary for our well-being; whilst even Sir William Herschel had attempted to find an explanation in justification of the time-honoured conception that the body of the sun might be at a low temperature and inhabitable by beings similar to ourselves, which he did in surrounding the inhabitable surface by a non-conducting atmosphere—the penumbra—to separate it from the scorching influence of the exterior photosphere.

It was not till the views of Kant, the philosopher, had been developed by La Place, the astronomer, in his famous 'Mécanique Céleste,' that the opinion gained ground that our central orb was a mass of matter in a state of incandescence, representing such an enormous aggregate as to enable it to continue radiation into space for an almost indefinite period of time.

The lecturer illustrated by means of a diagram the fact that of all the heat radiated away from the sun, only $\frac{1}{2250000000}$ part could fall upon the surface of our earth, vegetation and force of every kind being attributable to this radiation; whilst all but this fractional proportion apparently went to waste.

Recent developments of scientific research had enabled us to know much more of the constitution of the sun and other heavenly bodies than had formerly been possible. Comte says in his 'Positive Philosophy' (Martineau's translation of 1853) that "amongst the things impossible for us ever to know was that of telling what were the materials of which the sun was composed;" but within only seven years of that time Messrs. Bunsen and Kirchhoff published their famous research, showing that by connecting the dark Fraunhofer lines of the solar spectrum with the bright lines observed in the spectra of various metals, it was possible to prove the existence of those substances in the solar photosphere. thus laying the foundation of spectrum analysis, the greatest achievement of modern science.

Dr. Huggins and others, applying this mode of research to other heavenly bodies, including the distant nebulæ, had extended our chemical knowledge of them in a measure truly marvellous.

Solar observation had thus led to an analytical method by which chemistry had been revolutionised; and it would be, in the lecturer's opinion, through solar observation that we should attain to a much more perfect conception of the nature and effect of radiant energy in its three forms of heat, light, and actinism, than we could as yet boast of. The imperfection of our knowledge in this respect was proved by the circumstance that whereas some astronomers and physicists, including Waterston, Secchi, and Ericsson, had, in following Sir Isaac Newton's hypothesis, attributed to the sun a temperature of several millions of degrees Centigrade, others, including Pouillet and Vicaire, in following Dulong and Petit, had fixed it below 1500° C. Between these two extremes, other determinations, based upon different assumptions, had fixed the solar temperature at between 60,000° and 9000°.

The lecturer having conceived a process by which solar energy may be thought to a certain extent self-sustaining, had felt much interested for some years in the question of solar temperature. If the temperature of the solar photosphere should exceed 3000° C., combustion of hydrogen would be prevented by the law of dissociation, as enunciated by Bunsen and Sainte Claire Deville; and his speculative views regarding thermal maintenance must fall to the ground. To test the question, he in the first place mounted a parabolic reflector on a heliostat with a view of concentrating solar rays within its focus, which, barring comparatively small losses by absorption in the atmosphere and in the metallic substance of the reflector, should reproduce approximately the solar temperature. By introducing a rod of carbon through a hole at the apex of the reflector until it reached the focus, its tip became vividly luminous, producing a light comparable to electric light. When a gas burner was arranged in such a way that the gas flame played across the focal area, combustion appeared to be retarded, but was not arrested, showing that the utmost temperature attained in the focus did not exceed materially that producible in a Deville oxy-hydrogen furnace, or in the lecturer's regenerative gas furnace, in which the limit of dissociation is also reached.

Having thus far satisfied himself, his next step was to ascertain whether terrestrial sources of radiant energy were capable of imitating solar action in effecting the decomposition of carbonic acid and aqueous vapour in the leaf-cells of plants, which led him to undertake a series of researches on electro-horticulture, extending over three years, a subject he had brought before the Royal Society and the Royal Institution two years ago. By these researches he had proved that the electric arc possessed not only all the rays necessary to plant-life, but that a portion of its rays (the ultra-violet) exceeded in intensity the effective limit, and had to be absorbed by filtration through clear

glass, which, as Professor Stokes had shown, produced this effect without interference with the yellow and other luminous and intense heat rays. He next endeavoured to estimate the solar temperature by instituting a comparison between the spectra due to different known luminous intensities. Starting with the researches of Professor Tyndall on radiant energy, supplementing them by experiments of his own on electric arcs of great power, and calling to his aid Professor Langley, of the Alleghany Observatory, to produce for him a complete spectrum of an Argand burner, he concluded that with the temperature of a radiant source, the proportion of luminous rays increased in a certain ratio; whereas in an Argand gas burner only $2\frac{1}{2}$ per cent. of the rays emitted were luminous and mostly red and yellow, the most brilliant portion of a gas flame emitted 4 per cent., as shown by Tyndall, the carbon thread of an incandescent electric light between 5 and 6 per cent., a small electric arc 10 per cent., and in a powerful 5000-candle electric arc as much as 25 per cent. of the total radiation was of the luminous kind. Professor Langley, in taking his photometer and bolometer up the Whitley mountains, 18,000 feet high, had proved that of the solar energy not more than 25 per cent. was luminous, and that the loss of solar energy sustained between our atmosphere and the sun was chiefly of the ultra-violet kind. These rays, if they penetrated our atmosphere, would render vegetation impossible, as proved by the lecturer's own experiments above referred to. It was thus shown that the temperature of the solar photosphere could not materially exceed that of a powerful electric arc, or, indeed, of the furnaces previously alluded to, leading him to the conclusion already foreshadowed by Sainte Claire Deville, and accepted by Sir William Thomson, that the solar temperature could not exceed 3000° C. The energy emitted from a source much exceeding this limit would no longer be luminous, but consist mainly of ultra-violet rays, rendering the sun invisible, but scorching and destructive of all life. The accompanying diagram (Fig. 1) of the spectra alluded to shows clearly the gradual advance of the luminous band, as marked by the letters A to H.

Not satisfied with these inferential proofs, the lecturer had endeavoured to establish a definite ratio between temperature and radiation, which formed the subject of a very recent communication to the Royal Society.* The experiment consisted in heating, by means of an electric current, a platinum or iridio-platinum wire, a metre long, and suspended between binding screws, as shown in the accompanying sketch (Fig. 2); the energy of the current was measured by two instruments—an electro-dynamometer, giving it in ampères, and a galvanometer of high resistance giving the electro-motive force between the same points in volts. The product of the two readings gave the volt-ampères, or Watts of energy communicated to the wire, and dispersed from it by radiation and convection. A reference to the

---

* 'Proc. of the Royal Society,' vol. xxxv. p. 166.

Fig. 1.

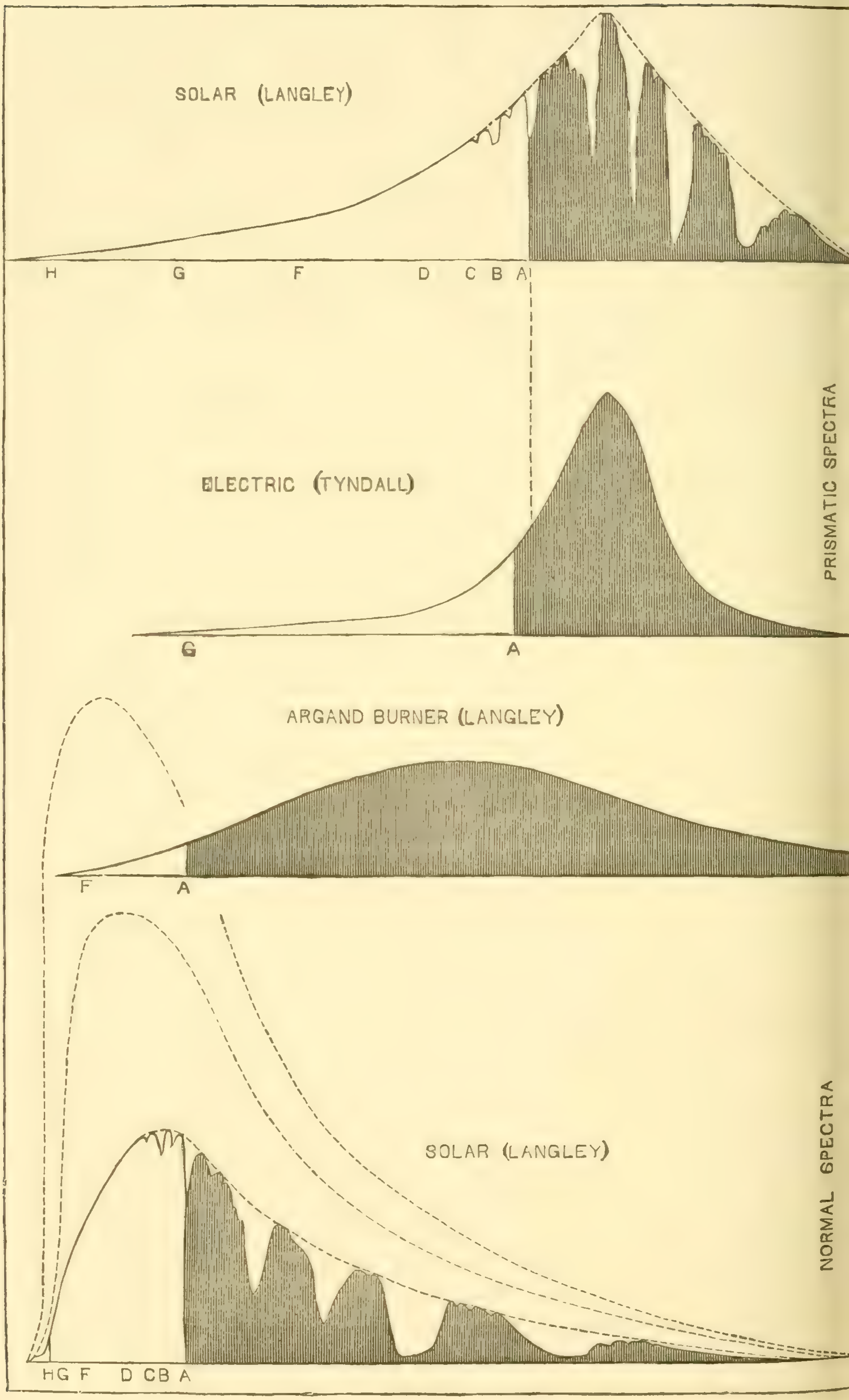

SOLAR (LANGLEY)
H G F D C B A
ELECTRIC (TYNDALL)
G A
PRISMATIC SPECTRA
ARGAND BURNER (LANGLEY)
F A
SOLAR (LANGLEY)
NORMAL SPECTRA
HG F D CB A

FIG. 2.

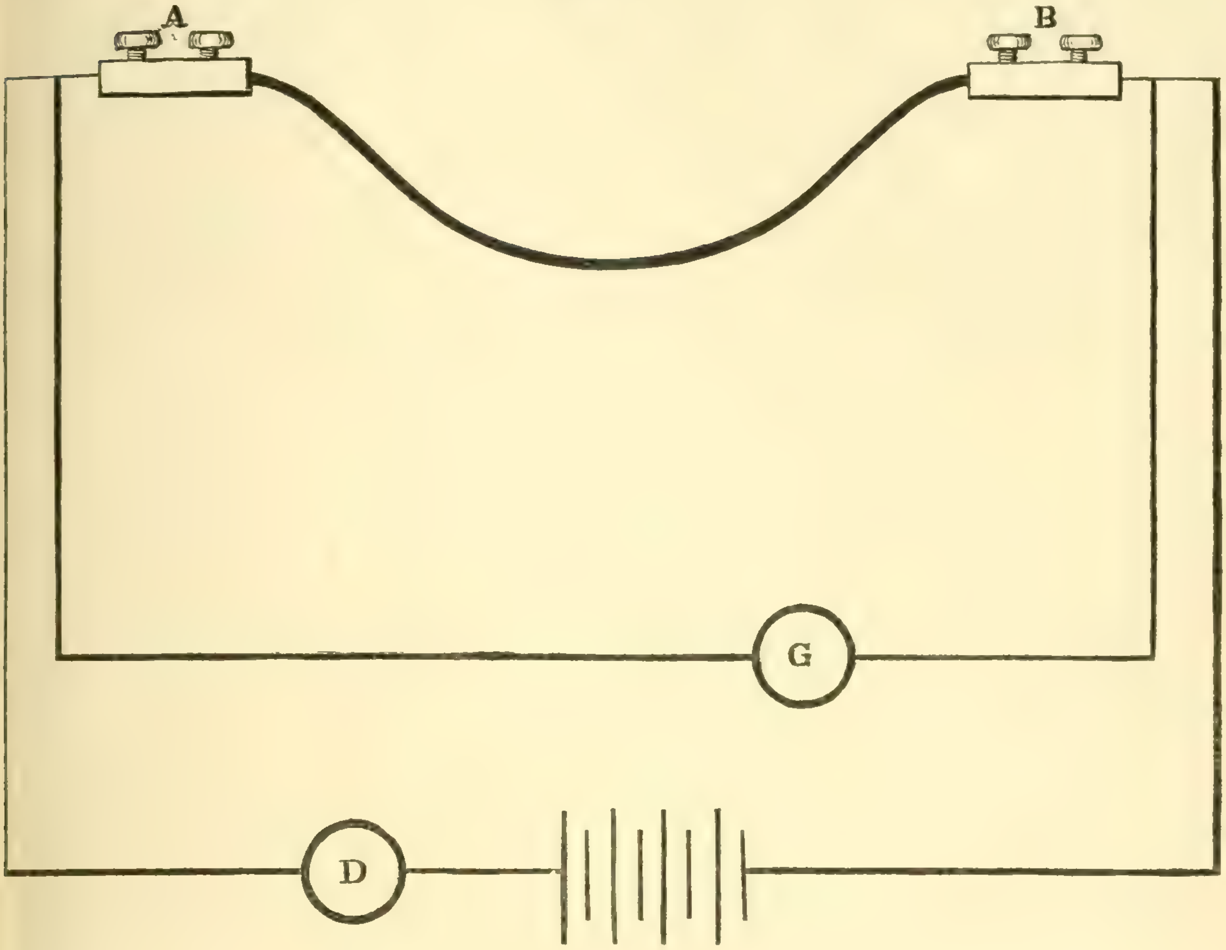
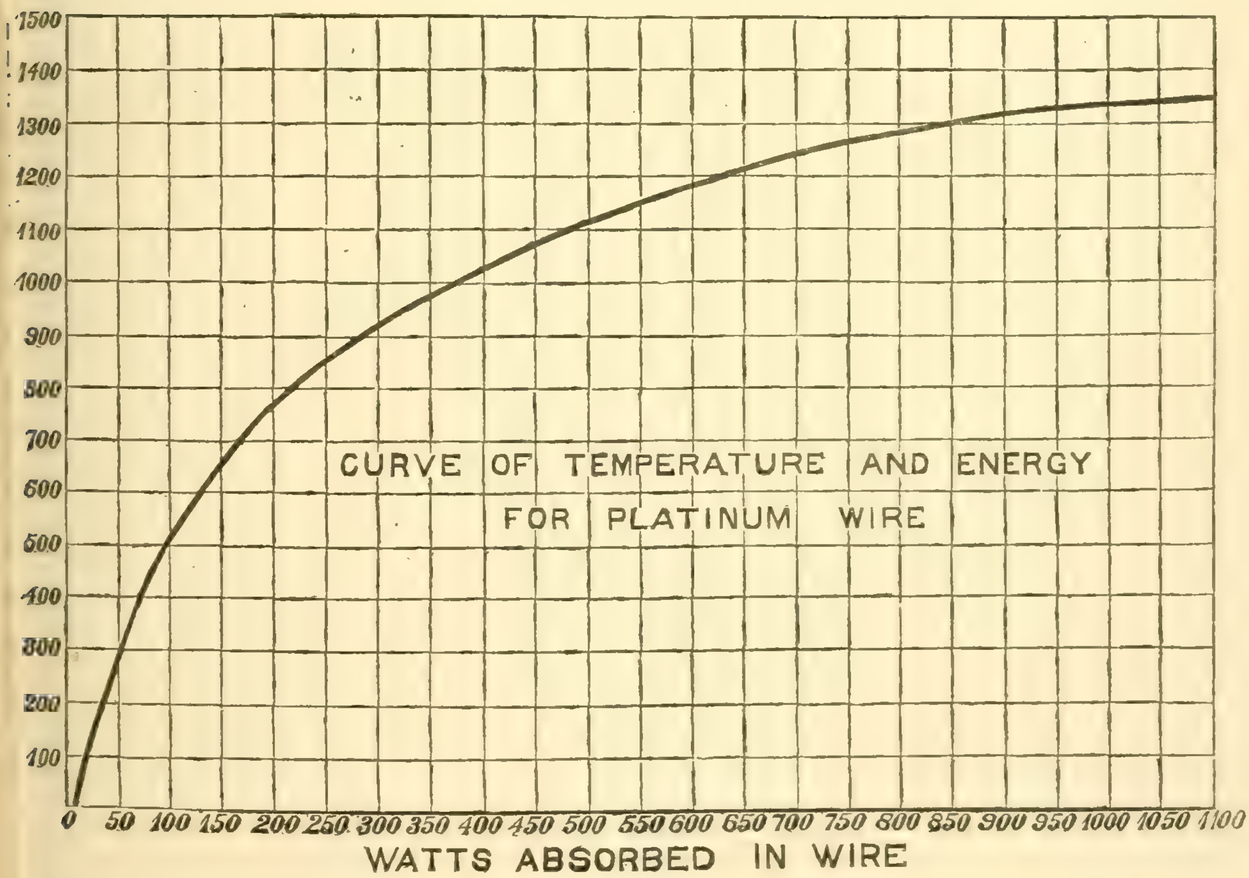

A
B
G
D

FIG. 3.


1500
1400
1300
1200
1100
1000
900
800
700
600
500
400
300
200
100
0
50 100 150 200 250 300 350 400 450 500 550 600 650 700 750 800 850 900 950 1000 1050 1100
CURVE OF TEMPERATURE AND ENERGY
FOR PLATINUM WIRE
WATTS ABSORBED IN WIRE

lecturer's paper on the Electrical Resistance Thermometer, which formed the Bakerian Lecture of the Royal Society in 1871, would show that the varying electro-motive force in volts observed on the galvanometer was a true index of the temperature of the wire while being heated by the passage of the current. By combining his former experiments on the dependence of resistance upon temperature, with his recent one, a law of increase of radiation with temperature was established experimentally up to the melting-point of platinum; this, when laid down in the form of a diagram, gave very consistent results expressible by the simple formula

$$\text{Rad}^{\text{tn}} = \text{M}\,t^2 + \phi\,t,$$

M being a coefficient due to substance radiating; an expression represented in the accompanying diagram (Fig. 3), in which the abscissæ represent energy dispersed and the ordinates the corresponding temperatures.

Sir William Thomson had lately shown that the total radiating energy from a unit of surface of the carbon of the incandescent lamp amounted to $\frac{1}{67}$th part of the energy emitted from the same area of the solar photosphere. and taking the temperature of the incandescent carbon at 1800° C. (the melting-point of platinum, which can just be heated to the same point), it follows in applying Sir William Thomson's deductions to the lecturer's formula that the solar photosphere does not exceed 2700° C., or, adding for absorption of energy between us and the sun, about 2800° C., a temperature already arrived at by the lecturer by a different method. The character of the curve was that of a parabola slightly tipped forward, and if the ratio given by that curve held good absolutely beyond the melting-point of platinum, it would lead to the conclusion that at a point exceeding 3000° C. radiation would become, as it were, explosive in its character, rendering a surface temperature beyond that limit physically difficult to conceive.

Clausius had proved that the temperature obtainable in a focus could never exceed that of the radiating surface, and Sainte Claire Deville that the point of dissociation of compound vapours rises with the density of the vapour atmosphere. Supposing interstellar space to be filled with a highly attenuated compound vapour, it would clearly be possible to effect its dissociation at any point where, by the concentration of solar rays, a sufficient focal temperature could be established; but it was argued that the higher temperature observable in a focal sphere was the result only of a greater abundance of those solar vibrations called rays within a limited area, the intensity of each vibration being the outcome of the source whence it emanated: thus, in the focal field of a large reflector the end of a poker could be heated to the welding-point, whereas in that of a small reflector the end of a very thin piece of wire only could be raised to the same temperature. If, however, a single molecule of vapour not associated or pressed upon by other molecules could be sent through the one focus

or the other, dissociation in obedience to Deville's law must take place irrespective of the focal area; but, inasmuch as the single solar ray represented the same potential of energy or period of vibration as numerous rays associated in a focus, it seemed reasonable that it should be as capable of dealing with the isolated molecule as a mere accumulation of the same within a limited space, and must therefore possess the same dissociating influence. Proceeding on these premises, the lecturer had procured tubes filled with highly attenuated vapours, and had observed that an exposure of the tubes to the direct solar rays or to the arc of a powerful electric light effected its partial or entire dissociation; the quantity of matter contained within such a tube was too slight to be amenable to direct chemical test, but the change operated by the light could be clearly demonstrated by passing an electric discharge through two similar tubes, one of which had, and the other had not, been exposed to the radiant energy from a source of high potential. If space could be thought filled with such vapour, of which there was much evidence in proof, solar rotation would necessarily have the effect of emitting such vapour equatorially by an action of circulation which might be likened to that of a blowing fan. When reaching the solar photosphere, by virtue of solar gravitation this dissociated vapour would, owing to its increased density, flash into flame, and could thus be made to account in great measure for the maintenance of solar radiation, whilst its continual dissociation in space would account for the continuance of solar radiation into space without producing any measurable calorific effect.

Time did not permit him to enter more fully into these subjects, which formed part of his solar hypothesis, his main object on this occasion having been to elucidate the point of cardinal importance to that hypothesis, that of the solar temperature.

[W. S.]

ANNUAL MEETING,

Tuesday, May 1, 1883.

WARREN DE LA RUE, Esq. M.A. D.C.L. F.R.S. Vice-President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1882, testifying to the continued prosperity and efficient management of the Institution, was read and adopted. The Real and Funded Property now amounts to above 85,400*l.*, entirely derived from the Contributions and Donations of the Members.

Thirty-five new Members paid their Admission Fees in 1882.

Sixty-three Lectures and Twenty Evening Discourses were delivered in 1882.

The Books and Pamphlets presented in 1882 amounted to about 206 volumes, making, with 517 volumes (including Periodicals bound) purchased by the Managers, a total of 723 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Honorary Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

PRESIDENT—The Duke of Northumberland, D.C.L. LL.D.
TREASURER—George Busk, Esq. F.R.S.
SECRETARY—William Bowman, Esq. LL.D. F.R.S.

MANAGERS.

George Berkley, Esq. M.I.C.E.
Sir Frederick J. Bramwell, F.R.S.
Warren De La Rue, Esq. M.A. D.C.L. F.R.S.
Dyce Duckworth, M.D. F.R.C.P.
Edward Frankland, Esq. D.C.L. F.R.S.
Colonel James Augustus Grant, C.B. C.S.I. F.R.S.
Right Hon. the Lord Claud Hamilton, J.P.
William Huggins, Esq. D.C.L. F.R.S.
Sir Frederick Pollock, Bart. M.A.
Henry Pollock, Esq.
The Hon. Rollo Russell, F.M.S.
The Marquis of Salisbury, K.G. M.A. F.R.S.
Sir William Siemens, D.C.L. LL.D. F.R.S.
William Spottiswoode, Esq. M.A. D.C.L. Pres. R.S.
Sir T. Spencer Wells, Bart. Hon. F.R.C.S. Eng.

VISITORS.

John Birkett, Esq. F.L.S. F.R.C.S.
The Lord Brabazon.
Charles James Busk, Esq.
Arthur Herbert Church, Esq. M.A. F.C
Frank Crisp, Esq. LL.B. B.A. F.L.S.
Henry Herbert Stephen Croft, Esq. M
William Crookes, Esq. F.R.S.
George Howard Darwin, Esq. M.A. F.R
Rear-Admiral Herbert De Kantzow, R
Clinton T. Dent, Esq. F.R.C.S.
Rev. John Macnaught, M.A.
Sir Charles Henry Mills, Bart. M.P.
Hugo W. Müller, Esq. Ph.D. F.R.S.
Lachlan Mackintosh Rate, Esq. M.A.
John Bell Sedgwick, Esq. F.R.G.S.

WEEKLY EVENING MEETING,

Friday, May 4, 1883.

Warren De La Rue, Esq. M.A. D.C.L. F.R.S. Manager,
in the Chair.

Robert Henry Scott, Esq. M.A. F.R.S. Secretary
to the Meteorological Council.

Weather Knowledge in 1883.

Rather more than ten years since I had the honour of delivering a Friday Evening Lecture in this Theatre, and my subject on that occasion was somewhat similar to that which I have chosen for this evening. It was then "Recent Progress in Weather Knowledge," and to-night I must endeavour to lay before you the results of ten years' further experience of a work of which the complexity becomes daily more patent.

Dealing first with the forecasting of the character of seasons. In 1873 the theory of a connection between the Frequency of Sun-spots and the Weather had been recently promulgated, and appeared to promise most valuable results. It is universally admitted that the presence of spots on the solar surface indicates increased activity in his gaseous envelope, which ought to and must affect our atmosphere.

A connection between the Frequency of Sun-spots and the prevalence of Hurricanes in the Indian Ocean was the first phenomenon which was cited as a proof that such influence is traceable, and the statistics brought forward appeared to confirm the idea. The periodicity of Rainfall was the next subject studied, and as an outcome of the changes in rainfall distribution the recurrence of Famines has furnished the text for numerous reports and pamphlets. Another subject dealt with has been Temperature, but as to this, one set of investigators holds that years of sun-spot frequency correspond to years of excessive heat, while another set maintains that they correspond to years of excessive cold!

This discordance has been somewhat allayed by the suggestion that the causes which produce heat in one region produce cold in another. On the whole, however, it may be said that the precise mode in which the sun exercises his action on our atmosphere has not as yet been explained, and as far as the climate of Western Europe is concerned, the warmest adherents of sun-spot influence must admit that observations of the condition of the sun's surface cannot as yet be depended on as a sound basis for prophecy of coming weather.

That this assertion cannot well be disputed appears from the figures which follow.

WINTER HALF-YEAR.

—	Temperature.	No. of Sun-spots.
	°	
1876–7	43·7	13
1877–8	43·4	8
1878–9	39·3	3
1879–80	41·4	24
1880–1	40·0	44
1881–2	43·7	64
1882–3	41·8	74

The second column gives the mean temperature of the entire United Kingdom for the six months October—March, for the last seven years, and the third gives the number of sun-spots observed at Kew during the same period.

No approach to concordance is traceable between the two columns of figures.

If we go further afield and compare the general temperature of the globe, at least the closest approximation to it which is attainable, with the sun-spot curve for the thirty-five years ending with 1875, as has been done by Dr. Köppen,* we see that though for some part of the time some of the temperature curves appear to agree with the curve of sun-spots, the accordance in one hemisphere is associated with a striking discordance in the other. (Fig. 1, p. 325.)

The figures which I have cited therefore support the statement that the precise nature of the relation between what we may call solar and terrestrial weather has not as yet been demonstrated.

As regards the whole question of prediction of the Seasons, either by sun-spots or by any other means, the same author, Dr. Köppen, has published several papers "On Protracted Periods of Weather," devoting his attention especially to severe winters, and he gives the following summary of his results :—†

"The main feature of the entire investigation has been to prove that, for certain intervals, strongly marked periodical influences make their appearance and then vanish entirely, at times being replaced by others of a totally different character. No law has as yet been discovered for these changes, and so the outcome of the enquiry is on the whole negative and indicates that all forecasting of the seasons is the merest guesswork."

We may therefore conclude that at the present date there is no

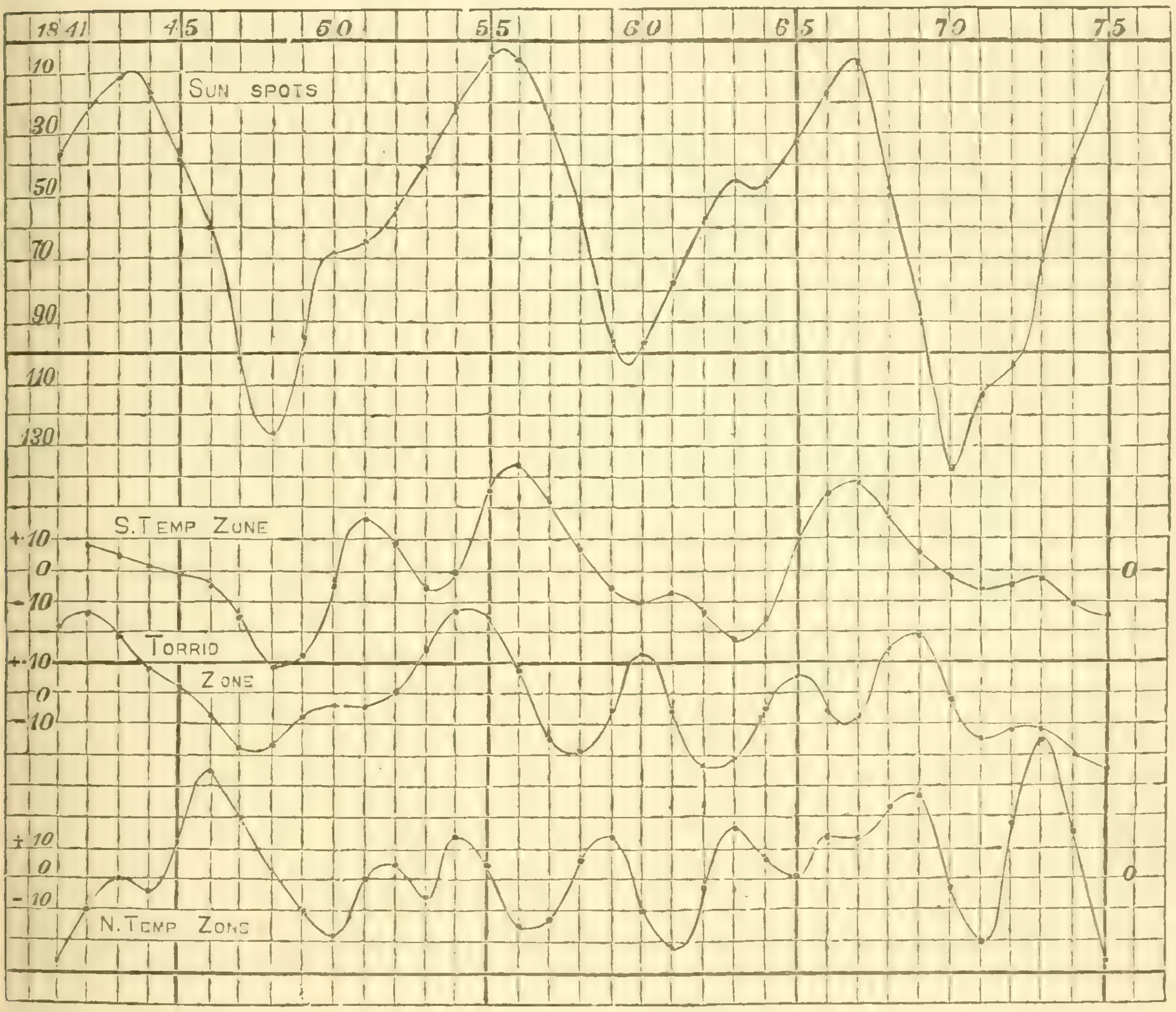
* 'Zeitschrift der österreichischen meteorologischen Gesellschaft,' Bd. xvi. p. 149.

† *Ibid.* p. 196.

immediate prospect of any one being able to state what the character of a future season will be, much less to tell a farmer in spring what crops he should put in with a prospect of a favourable period for the harvest.

I next come to the subject of daily forecasting, a branch of the work of all Meteorological Institutes, which has grown up in Europe within the last ten years. It has really been forced upon meteoro-

FIG. 1.



NOTE.—The vertical scale in the upper part of the diagram refers to number of sun-spots; in the lower part it refers to differences from average temperature in tenths of degrees Centigrade.

logists by the demand of the public to see in the newspapers some statement as to probable weather.

The brilliant successes achieved in this line by the Chief Signal Office at Washington have attracted general notice, and the public of every nation in Europe has expected that their own offices shall do as much for them as that of Washington does for the States of the Union.

The public has, however, naturally forgotten to take into consideration several most important advantages which the Signal Office

enjoys over its compeers. Firstly, an extensive continent from which to gather its reports, with one language and one telegraphic system; secondly, a military organisation, which ensures due training and proper subordination of the employés to their chiefs; and thirdly and finally, a most liberal supply of funds.

In all these particulars European systems fall far behind that of the United States. Our continent is not large, and it is cut up by inland seas like the Mediterranean and the Baltic, while for us in these islands, the ocean lies to the westward, and from that we can at present gather no reports.

Moreover, the difference of languages and habits between the different states has been found, hitherto, to present insuperable difficulties to the introduction of an absolutely uniform system of reporting.

To take one instance of the difficulty of organising weather reports in Europe, information as to the state of Sea Disturbance on our shores is justly considered one of the most important observations for our coast observers, but when we came to devise an international code for reporting, we found that inland organisations, such as that of Austria, objected to setting apart any space in the code for such, to them, uninteresting details.

My audience will therefore see that it is not easy for us Europeans to devise a system of weather reporting which shall meet with universal acceptance.

Again, supposing that the code were satisfactorily arranged as regards the reports of instrumental observations, it is found to be practically impossible to organise a system of notices of the general appearance of the sky and the weather, in fact, of the very indications which are the most valuable of all to the skilled weather watcher.

Here is the crowning defect of all centralised weather services like our own; the forecaster, situated at a distance of some hundred miles from his most important stations, has to draw his conclusions at second hand, from information at best scanty for each station, frequently unpunctual in its arrival, and also at times entirely deficient at the most critical moment, owing to interruptions in telegraphic communication during stormy weather.

The only practical mode of partially overcoming this difficulty of excessive centralisation, would entail very considerable expense. It would be the maintenance in the chief centres of population, of local offices, charged with the preparation of forecasts for their own special neighbourhoods, and I fear that neither the Government nor the local authorities would give money for such an experiment.

No serious attempt at local forecasting is made in Europe, except by one German newspaper, the *Magdeburger Zeitung*, for the scheme started in France by Leverrier shortly before his death, of local forecasting by experts appointed by the Communal Authorities, was speedily brought to a stop after his death by a demand from the French Postal Authorities for the repayment of the expense of telegraphy.

We are thus obliged to forecast for the whole country, and as the

various portions of these islands are variously circumstanced as regards proximity to the sea, and as to the mountainous or flat character of their surface, an attempt has been made to group the counties together into a series of districts, the boundaries of which are determined by the general character of the agriculture most developed within them. Thus the western districts are mainly grass producing, while the eastern are corn producing.

This division is necessarily more or less of an arbitrary nature, as the separation between a grazing and an arable region is not a hard and fast line, and of course a driving shower does not cease to fall on crossing a county boundary. But supposing a forecast is drawn up for any one district, it is necessarily limited by the exigencies of telegraphy to a very few words, and it is simply impossible to frame it so as to be correct for the whole of the district. If there is a range of hills crossing the country, the wind which produces rain on the weather slope of the ridge brings dry weather to its lee side. This is a consequence of the well-known principle, that air forced to rise over an obstacle like a hill, is cooled at the rate of about 1° F. for every 300 feet, and is also expanded by the pressure on it being reduced. Both of these actions reduce its capacity for containing moisture in the state of vapour, and rain is produced as the air ascends. Once it has crossed the ridge the reverse action takes place, the air is heated and compressed by descent. It thus becomes capable of containing more moisture, and is felt as a warm and dry wind.

To give an instance of this action on a somewhat large scale, I may cite the district of Elgin and Nairnshire, on the south coast of the Moray Firth. This comparatively flat area is bounded on the south and south-east by the Grampians, and on the west by the hills of Inverness-shire and Ross-shire, and all the air from the south and west has had to travel up a series of successive hill sides on its passage from the Atlantic. The result is, that the region I have named has a rainfall not more than one half that of the upper valleys of the Grampians, and yet it is situated in the same district for forecasting. Such is the exceptionally dry character of this strip of coast, that one summer a friend of mine who has a fishing on the Spey told me that for some weeks while the river was in spate, showing that rain fell on the hills, and the fishermen out in the Firth had rain almost every night, not a drop fell at Nairn

My hearers will therefore perceive that the same wording will not suit a whole district, unless it be judiciously phrased so as to bear more than one interpretation.

The figures which I give in Table I. are obtained in a very general way; the forecast for the district is tested by as many reports from that district as are available. This mode, therefore, gives a higher figure for correctness than would be obtained by testing them for a single place.

The second series of figures, in Table II. however, are obtained by

a different method, and one which is comparatively free from the objection just stated, as the observers who furnish the data are quite independent of the office, and the forecast is tested by the weather they themselves experience.

TABLE I.—FORECASTS AT 6 P.M., APPEARING NEXT MORNING.

	Quite correct.	Partially, more than half, correct.	Partially, more than half, wrong.	Quite wrong.	Sum of Cols. I. and II.
Scotland, N.	38	41	15	6	79
,, E.	36	43	15	6	79
England, N.E.	35	42	17	6	77
,, E.	37	42	15	6	79
Midland Counties ..	34	43	17	6	77
England, S.	40	42	13	5	82
Scotland, W.	31	40	20	9	71
England, N.W. ..	33	42	18	7	75
,, S.W. ..	35	40	18	7	75
Ireland, N.	34	42	17	7	76
,, S.	35	39	17	9	74
General average ..	35	41	17	7	76

TABLE II.—HAY HARVEST FORECASTS.

Scotland, N.	44	38	15	3	82
,, E.	44	38	14	4	82
England, N.E. ..	38	37	20	5	75
,, E.	46	34	15	5	80
Midland Counties ..	48	37	13	2	85
England, S.	43	40	14	3	83
Scotland, W.	36	39	15	10	75
England, N.W. ..	39	38	19	4	77
,, S.W. ..	42	37	16	5	79
Ireland, N.	32	39	22	7	71
,, S.	39	35	21	5	74
General average ..	41	37	17	5	78

These latter are the results of the Hay Harvest Forecasts, a system which has been in operation for the past four years. Forecasts drawn at 4 P.M. are sent daily for a month to a number of gentlemen, largely interested in farming, in various parts of the country, on the conditions that they disseminate the information in their immediate neighbourhoods, and that they keep and send to us a careful comparison of the forecasts with the weather. The general average of

the whole agrees sufficiently closely with that determined by the office from its own data.

I find, on enquiry, from all the European offices which issue forecasts, that the percentages of success which they claim officially are almost identical with that shown on the diagrams. They are on the whole about 80, but no one is really contented with the results. The critics of foreign forecasts are just as severe on their own systems as the writers of newspaper letters are on us over here, and as one of my correspondents, the gentleman who manages the Magdeburg office remarks, those who are least content are the forecasters themselves, though naturally they do not publish their dissatisfaction.

To give my audience one illustration out of many of the difficulty of our task in England, the same gentleman whom I have just quoted says: "Our greatest trouble is the lateness of arrival of the English telegrams, and, without news from England, no one in Germany can dream of forecasting." Now we, in these islands, as is so often said, can apparently never hope for daily telegraphic news from signal ships in the Atlantic, so that ocean must keep its wonted silence, and our forecasting must be even more hazardous than that of our German neighbours.

Another development of forecasting, and in fact its earliest form, the desire to carry out which gave rise to the whole system of weather telegraphy, and as a result of the latter, to the science of Modern Meteorology, is the issue of Storm Warnings. These were instituted in this country by FitzRoy, in 1860. They were temporarily suspended at Christmas 1866 and resumed a year later, and the diagram gives the average results for the entire United Kingdom, from the year 1870, being the first year for which the figures were regularly submitted to Parliament. The interval of twelve years divides itself naturally into two periods of equal length, the break between which happens to coincide with the institution of an evening, in addition to the afternoon service of reports.

This was carried on, in the first instance, at the sole cost of the *Times* newspaper, and in order to furnish materials for drawing the chart in its morning issue, and it is only within the last three years that the expense of this part of our work has been borne by the Government.

The figures show at the first glance an improvement of 8 per cent. in the first column, and this is a considerable advance, but my audience must not take away the idea that if the receipt of four hours' later intelligence raises the percentage of success, we might anticipate the possibility of warning the coasts for all storms, if the reporting hours were extended to midnight, or the service were even continuous.

The extension of the reporting service would enable us in London to know what was taking place on the coasts far better than we do, but we could not impart our knowledge to those whom we wish to benefit. Practically, storm warnings must be issued before sundown,

for no port at present will bear the expense of maintaining lamps for night signals. If we allow an hour or so for the warning message to reach the distant stations, and most of them take much longer than that, we see that in winter a warning to a Scotch port must leave London at 2 P.M. to be in time to be communicated by signal. Warnings issued at 7 P.M. rarely come to the fisherman's knowledge till next morning, even if they should reach the telegraph station before that office closes for the night.

Accordingly the figures in the table give a somewhat too favourable idea of our real success in warning.

—	Warnings justified		Warnings not Justified.	Warnings		Warnings issued in error.
	By Gales.	By Strong Winds.		Late.	Partially late.	
1870–5	47·6	26·8	18·4	1·5	3·8	1·9
1876–81	56·4	23·5	15·8	0·8	3·1	0·3
1870–81	52·0	25·1	17·1	1·2	3·5	1·1

The diagram also does not show the storms which have been missed. Of these there are instances every year. That of October 23–4, 1882, was a most striking case. The storm came on so suddenly, not setting in at any station before midnight, and raging with full fury at 8 A.M., that with our present knowledge it appears to have been impossible to have caught it.

It would take me too long were I to continue this subject, and I would only impress upon my hearers that accidents must happen like that of October last, and that we must only not let them discourage us, and do our honest best.

The practical result of our forecasting of weather is that while we are generally fairly correct as to the direction and force of wind, we are most liable to fail in predicting rain, especially as to its amount. In fact not only we, but every Meteorological Office in Europe, have to confess inability to foretell rain, *quantitatively*, to say whether the rain expected to fall will be only slight, or a deluge. In no single case have exceptionally heavy falls, either local, like thunderstorms, or general rains, been foretold. I take as instances, the rain of April 13, 1878, in London, which burst so many sewers; the hail-storm at Richmond, August 3, 1879, which will long be remembered in Kew Gardens; or lastly, the snow-storm of January 18, 1881.

These failures are very serious defects in practice, and apparently are in great measure attributable to our own ignorance of the conditions of the upper strata of the atmosphere.

Attempts have been made in various directions to organise systems

of upper strata observations. These may be classified under three heads, *Personal*, *Mechanical*, and *Optical*.

By *personal observations*, I mean those made on mountain stations or in balloon ascents.

Of Mountain Stations we have not yet had a fair trial in these islands, for no mountain observatory, deserving the name, has yet been built. The Scottish Meteorological Society are endeavouring, with great zeal and considerable success, to raise funds to build and maintain a station on Ben Nevis, the highest spot in the United Kingdom, and to place it in telegraphic connection with Fort William, and so with the postal telegraph wires in general, but hitherto all that has been done is that the observer, Mr. C. L. Wragge, has with a most praiseworthy exertion of energy, and in the face of great difficulties, climbed the 4000 feet before 9 A.M. *daily* for $4\frac{1}{2}$ months in 1881, and for 5 months in 1882. To give an idea of what he occasionally experienced, I may quote his words in a letter of November 1, 1882, printed in 'Nature': "The track was snowed up, and it was necessary to force a way through great banks and drifts of snow. The average depth was 2 feet; once we got off our course in the blackness of thick cloud-fog, and trackless snow."

Mr. Wragge on each occasion, as soon as he reached Fort William on his return, generally about 3 P.M., sent a telegram to us in London. This never arrived before 5 P.M., so that it was practically useless for all forecasting or storm warning on the day on which the observation was made. We must therefore reserve our judgment as to the usefulness of the proposed station until it is in efficient working order, and the observations can reach us more promptly.

As regards Balloons the observations are necessarily sporadic and uncertain. No ascent is possible if the wind is at all strong. No captive ascent can attain any great height, and no free ascent can be made with a certainty of being able to send off a telegraphic message when the observer reaches *terra firma*, for he may come down miles from a telegraph station.

The admitted impracticability of guiding a balloon, and the liability to accidents, in the case of a squall, either on leaving the ground or on returning to it, of both of which recent instances, one unfortunately fatal, are on record, render it unadvisable to rest any hopes of permanently extending our knowledge of the upper currents of the atmosphere by the aid of aeronauts. Moreover, they can never give us reports at a fixed hour every day for the purpose of completing our charts.

The *mechanical* method of observation may be soon dismissed: it consists either in sending up instruments in small captive balloons, or attached to kites, or in placing them on elevated peaks. In all these cases the registration is to be effected by means of electricity. The apparatus devised by Sir W. Siemens, and lent by him to the Meteorological Society, has been in operation on Boston church tower for several months, and has worked well, and the same may be said

of Olland's Telemeteorograph on the tower of Utrecht Cathedral, but the obtaining of records from such an elevation as can be secured on the loftiest buildings, is not obtaining them from the upper currents of the air, and we have yet to prove the practicability of raising an electric thermometer to a height of, say, 2000 feet, and maintaining it there in all ordinary weathers, before we can say that much is to be expected from mechanical upper current observation.

Lastly, we come to *optical* modes of observing the condition of the air above us. These are the only ones which as yet give us much encouragement, and of them I shall only mention two. Spectroscopy and Cirrus Cloud Observations.

The former has an enthusiastic advocate in Professor Piazzi Smyth, whose repeated letters to the newspapers have at least attracted the notice of many who have not seen his copiously illustrated work on the subject, "Madeira Spectroscopic." Professor Smyth maintains that by observations of his rain-band in the spectrum, he can form an accurate estimate of the amount of moisture suspended in the air. This belief of his is not as yet, however, accepted as an article of faith by meteorologists at large, and, even if it were, it still remains to be proved how much warning of coming rain such phenomena will afford. If they only give it for a few hours, the advantage they present to us is not very material.

Under any circumstances there are great obstacles to the general introduction of spectroscopic observations at telegraphic reporting stations. The instruments are comparatively costly, and their use requires more skill and delicacy of handling than we can expect from men of the rank of our ordinary telegraphic reporters.

Lastly, we come to the observation of the clouds, especially of the upper clouds, which has been of late almost reduced to a science, mainly by the labours of the Rev. W. Clement Ley in this country, and of Professor H. Hildebrandsson of Upsala in Sweden, around whom a knot of observers are gathering. Mr. Ley is the most enthusiastic, and also by far the most experienced of the authorities upon the subject, and he said of himself in a lecture delivered a few years ago, that he had spent $\frac{1}{12}$ part of his waking existence in watching cloud motion. What he advances therefore must be received with due respect.

For his own district, the Midlands, he claims to be almost infallible as regards weather, when he can secure an observation, and from our experience of his telegrams to us his announcements for the country in general are frequently astonishingly correct. In few words, the principle which he applies is the motion of the highest stratum of clouds, the "cirrus" of Howard and its relatives. He has shown that the various motions of these clouds can be explained by the view that the air slowly whirls out of a cyclonic area in the upper strata, in directions opposite to those of the wind motion at the earth's surface.

Thus for instance, when pressure is higher over France or Germany

than it is in Scotland, a motion of cirrus from north-west indicates the existence of a depression situated to the west of us, and as that depression advances on us the first wind we shall feel will be south or south-east, certainly not north-west.

Similar rules have been laid down for cirrus motion in other azimuths, and from its rate conclusions may be drawn as to the motion of the depression whence it takes it rise.

This seems exceedingly promising, but now comes the other side of the picture. Mr. Ley himself admits that the faculty of cloud observing is incommunicable by simple teaching. The motions of these clouds are so gradual, and are apparently so liable to be confounded with the motions of other and lower strata, that a great exercise of judgment is requisite before an opinion is pronounced.

Supposing, even, we have only one stratum to deal with, the head must be kept immovable during the period of observation, and the motion of the cloud across or past a fixed object, like a chimney, watched. Lastly, the observations cannot be always taken at fixed hours, at the fixed observing epochs, but must be made whenever opportunity offers. The observers, therefore, must have abundance of leisure, and that is a commodity hard to meet with in these busy times.

What is, then, the general conclusion we can draw as to weather knowledge and its prospects in 1883? I have endeavoured to show you that *weather knowledge* is practically *weather prediction*, and that, for the seasons, in Europe at least, no trustworthy basis for prediction has as yet been established.

For the daily forecasting of weather much has been effected, but much more remains to be done, and the most important advance we can make is in the direction of training observers in the difficult art of upper cloud observation, the most promising field of study at present.

The enquiry into Atlantic weather which is now being carried on in our Office, and which enables us to prepare daily weather maps of the ocean with nearly 400 observations on each, will, it is hoped, throw light on how and where some of the storms which visit us take their rise, but it will certainly show what actual condition over the ocean accompanied each manifestation and movement of cirriform clouds at our stations, and will thus afford data for laying down rules to determine the position and track of a storm-centre before it reaches our coasts.

Cirrus observations are in fact the only practical means we have of, so to speak, annulling our insular and isolated position, and of extending our outposts over the Atlantic.

[R. H. S.]

GENERAL MONTHLY MEETING.

Monday, May 7, 1883.

GEORGE BUSK, Esq. F.R.S. Treasurer and Vice-President in the Chair.

The following Vice-Presidents for the ensuing year were announced:—

Sir Frederick J. Bramwell, F.R.S.
Warren De La Rue, Esq. M.A. D.C.L. F.R.S.
Sir Frederick Pollock, Bart. M.A.
The Marquis of Salisbury, K.G. M.A. F.R.S.
Sir William Siemens, D.C.L. LL.D. F.R.S.
William Spottiswoode, Esq. M.A. D.C.L. Pres. R.S.
George Busk, Esq. F.R.S. Treasurer.
William Bowman, Esq. LL.D. F.R.S. Honorary Secretary.

W. Mitchell, Esq.
Miss Orbell.
Martin Ridley Smith, Esq.

were elected Members of the Royal Institution.

John Tyndall, Esq. D.C.L. LL.D. F.R.S. was re-elected Professor of Natural Philosophy.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Geological Survey of India: Records. Vol. XVI. Part 1. 8vo. 1883.
The Secretary of State for India—Report on Public Instruction in Bengal, 1881–2. fol. 1883.
Accademia dei Lincei, Reale, Roma—Atti, Serie Terza. Vol. VII. Fasc. 5, 6. 4to. 1883.
Asiatic Society of Bengal—Journal, Vol. L. Extra Number, Part 1. 8vo. 1882. Proceedings, No. 10. 8vo. 1882.
Astronomical Society, Royal—Monthly Notices, Vol. XLIII. No. 5. 8vo. 1883.
Bankers, Institute of—Journal, Vol. IV. Part 5. 8vo. 1883.
British Architects, Royal Institute of—Proceedings, 1882–3, Nos. 12, 13. 4to.
British Association for the Advancement of Science—Report of Meeting at Southampton, 1882. 8vo. 1883.
British Museum, Trustees of the—Catalogue of Greek Coins. Thessaly to Aetolia. And the Ptolemies, Kings of Egypt. 2 vols. 8vo. 1883.
Candolle, M. C. de, M.R.I. (the Author)—Rides sur le sable déposé au fond de l'eau. (Archives des Sciences. T. IX. 1883.) 8vo. 1883.

Chemical Society—Journal for April, 1883. 8vo.
Civil Engineers' Institution—Minutes of Proceedings, Vol. LXXI. 8vo. 1883.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (*the Editor*)—Journal of the Royal Microscopical Society, Series II. Vol. III. Part 2. 8vo. 1883.
Dax: Société de Borda—Bulletins, 2e Serie, Huitième Année: Trimestre 1. 8vo. 1883.
East India Association—Journal, Vol. XV. No. 1. 8vo. 1883.
Editors—American Journal of Science for April, 1883. 8vo.
Analyst for April, 1883. 8vo.
Athenæum for April, 1883. 4to.
Chemical News for April, 1883. 4to.
Engineer for April, 1883. fol.
Horological Journal for April, 1883. 8vo.
Iron for April, 1883. 4to.
Nature for April, 1883. 4to.
Revue Scientifique and Revue Politique et Littéraire for April, 1883. 4to.
Telegraphic Journal for April, 1883. fol.
Franklin Institute—Journal, No. 688. 8vo. 1883.
Geographical Society, Royal—Proceedings, New Series, Vol. V. No. 4. 8vo. 1883.
Geological Society—Abstracts of Proceedings, 1882-3, Nos. 436, 437. 8vo.
Harlem, Société Hollandaise des Sciences—Archives Néerlandaises, Tome XVII. Liv. 3, 4, 5. Tome XVIII. Liv. 1. 8vo. 1882–3.
Hudleston, Wilfrid H. Esq. M.A. F.G.S. M.R.I. (*the Author*)—On the Geology of Palestine. 8vo. 1883.
Johns Hopkins University—American Journal of Philology, No. 12 Sup. 8vo. 1883.
American Chemical Journal, Vol. IV. No. 6. 8vo. 1883.
Linnean Society—Journal, Nos. 97, 98, 126, 127. 8vo. 1883.
Transactions: Botany, Vol. II. Parts 2–4. Zoology, Vol. II. Part 6. 4to. 1882–3.
Manchester Geological Society—Transactions, Vol. XVII. Part 6. 8vo. 1882–3.
Mechanical Engineers' Institution—Proceedings, No. 1. 8vo. 1883.
Medical and Chirurgical Society—Proceedings, New Series, No. 2. 8vo. 1883.
National Association for Social Science—Proceedings, Vol. XVI. No. 3. 8vo. 1882.
Transactions, Nottingham, 1882. 8vo. 1883.
Pharmaceutical Society of Great Britain—Journal, April, 1883. 8vo.
Photographic Society—Journal, New Series, Vol. VII. No. 6. 8vo. 1883.
Political Economy Club—List of Members, 1821–1882: Minutes, Questions discussed, &c. Vol. IV. 8vo. 1882.
Preussische Akademie der Wissenschaften—Sitzungsberichte, XXXIX–LIV. 4to. 1882.
Royal Society of London—Proceedings, No. 224. 8vo. 1883.
Scottish Society of Arts, Royal—Transactions, Vol. X. Part 5. 8vo. 1883.
Siemens, Sir William, D.C.L. LL.D. F.R.S. M.R.I. (*the Author*)—On the Conservation of Solar Energy. 8vo. 1883.
Society of Arts—Journal, April, 1883. 8vo.
Statistical Society—Journal, Vol. XLVI. Part 1. 8vo. 1883.
Symons, G. J. Esq. F.R.S.—Monthly Meteorological Magazine, April, 1883. 8vo.
Telegraph Engineers, Society of—Journal, Vol. XI. No. 47. 8vo. 1883.
Teyler Museum—Archives, Série II. 3e Partie. 4to. 1882.
University of London—Calendar, 1883. 8vo.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1883: Heft 3. 4to.
Vincent, Benjamin, Esq. Assist.-Sec. and Librarian, R.I.—A Treatise on the Lord's Supper. By John Glas. With Biographical Preface. 8vo. 1883.

WEEKLY EVENING MEETING,

Friday, May 11, 1883.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Professor Huxley, LL.D. F.R.S.

Oysters and the Oyster Question.

[The whole discourse, with additions, is printed in the October and November numbers of the 'English Illustrated Magazine,' published by Messrs. Macmillan.]

The oyster possesses representatives of all the most important organs of the higher animals, and is endowed with corresponding functions. The "loves of the oyster" may be mythical, and we may even be sceptical as to its parental tenderness; but no parent can take greater care of its young. And though the oyster seems the type of dull animal vegetation in its adult condition, it passes through a vagabond, if not a stormy youth, between the time in which it is sheltered by the parental roof, and that in which it "ranges itself" as a grave and sedentary member of the oyster community. It has a shell composed of two pieces or *valves*, the one of which is thick and has a convex outer surface, while the other is thinner and flattened. The contour of each valve is irregularly oval, with a small end, which usually presents a triangular prominence known as the *umbo* or beak (*um.*) (Fig. 1), and which answers to the back or dorsal region of the animal. When this is turned upwards, the opposite or ventral margin is seen to be evenly curved, and to be gradually continued into the curved line of the front margin (*ant.*), while the hinder margin (*post.*) is usually straighter. By attention to these characters, the right valve can always be distinguished from the left; but, in the great majority of cases, it is more easily known because it is the flat valve. If the oyster is fixed, it is the convex valve which is attached; and free oysters naturally lie on this valve, inasmuch as the other is the lighter, and the more easily raised by the mechanism which will be presently described.

The exterior of the shell is rough and usually of a brownish-green colour. It is marked by lines, which run approximately parallel with the contour of the shell, around a common centre placed at the summit of the umbo, and indicate the successive layers by the apposition of which the shell has been deposited upon the skin of the animal. The inner face of each valve has the well-known white and opaline or iridescent aspect which appertains to nacre or mother-of-pearl, except the flattened or concave surfaces of the umbones, which are marked by parallel lines answering to the lines of growth on the

exterior. At the bases of the umbones, the valves are joined together, along a short transverse line, in a sort of hinge by a band of dark-brown elastic substance, the *ligament* (*l.*), which, in the middle of the hinge, forms a thicker cushion. About the centre of the inner face of each valve there is a large well-defined rounded depression, like a scar, which marks the place of attachment of a strong and important muscle.

Each valve is sometimes solid throughout, but in old oysters, and especially in those that live in deep water, the substance of the valves, and more particularly of the thick convex left valve, contains wide cavities, separated only by thin layers of nacreous substance, which are full of sea-water.

In structure, the nacre, or mother-of-pearl, is very dense, hard, and finely laminated; but the superficial outer layer is made up of small polygonal prisms, and is somewhat friable. Each of these substances, the nacreous and the prismatic, consists of layers of organic matter impregnated with salts of lime.

If the oyster has been left at peace for some time in its native sea-water, the edges of the valves, beyond the hinge, will be seen to be separated by a chink which is wider opposite the umbones. But, upon the least disturbance, the chink is closed and the shut valves cannot be thrust asunder, without the expenditure of an amount of force which usually breaks them.

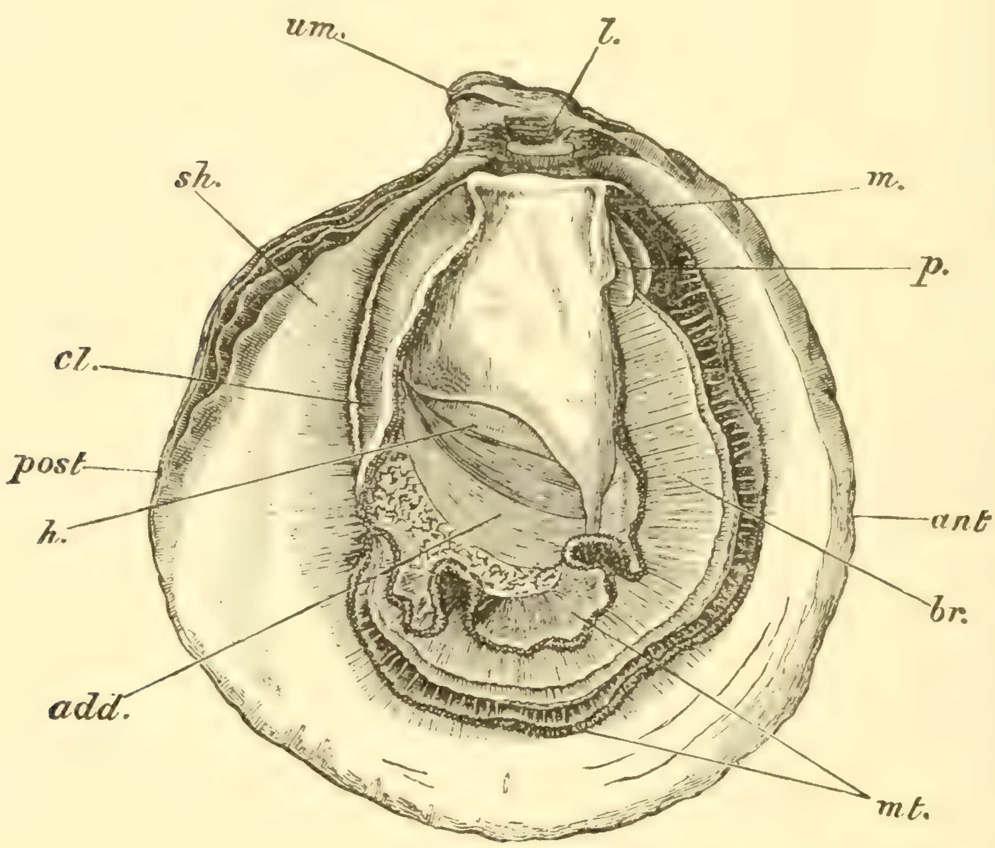
An expert oyster-opener, however, mindful of the maxim, *arte non vi*, gets them apart with the utmost ease. A strong, flat-bladed knife is introduced between the margins of the valves, and the knife being kept close to the inner face of one of them, is swept round the region of the muscular impression. If the operation is properly performed, the shell at once gapes widely; and it will now be found that, if the valve which has sprung up is pressed down, it immediately returns to its former position. The shell that, before, could hardly be forced open, now will not keep shut. The reason of this becomes apparent if the soft body, the edible part of the oyster, which lies within the shell is carefully cleaned out so that the interior of the valves can be seen. On looking towards the hinge, the thick elastic cushion formed by the middle of the ligament, will be found to be compressed when the valves are brought together; and, when the external pressure is removed, its elastic reaction suffices to thrust them apart. In fact, it is like the spring of a door, arranged in such a manner as to keep the door ajar. While the oyster is alive, the great muscle already mentioned, which is called the *adductor* (Fig. 1, *add.*), the ends of which are attached to the two scars on the inner face of the valves, is always ready to overcome the elasticity of the ligament and close the valves, when need arises. And what the judicious oyster-opener does is to cut this muscle close to one or other of its attachments. Thus the force by which the valves are made to gape is elasticity of a purely mechanical character, and is as active in the dead as in the living oyster; while that by which the valves are closed is the contractility

which inheres only in living muscle. Hence a dead oyster is readily known by its persistent gaping.

We shall see that a certain amount of separation of the valves is necessary for the discharge of all the functions of the living oyster. Hence, it is of advantage to the animal that this condition should be assumed and maintained without any muscular exertion; while intruders and enemies can be shut out, or at any rate sharply pinched, at any moment, by calling the adductor into action.

If one valve is removed very carefully, so as not to injure the soft body within, the form of the latter is seen to have a general correspondence with that of the interior of the shell. It is therefore flattened from side to side, with the left side convex and the right flattened (Fig. 1). Its contour is oval, with the long axis perpendicular to the middle of the hinge; and there is a short dorsal side

FIG. 1.



An Oyster with the right valve of the shell removed. Natural size.

sh. shell; *um.* umbo; *l.* ligament; *m.* mouth; *p.* palps; *br.* branchiæ; *mt.* mantle; *add.* adductor muscle; *h.* heart; *cl.* cloaca; *ant.* anterior; *post.* posterior side.

which answers to the latter, and is excavated in the middle, in correspondence with the convex ventral face of the cushion of the ligament. The dorsal half of the anterior edge of the body is convex, that of the posterior edge is nearly straight, or even slightly excavated, while the ventral margin continues back the curve of the anterior edge. The cut end of the thick adductor muscle is a conspicuous

object, and corresponds in shape with the adductor impression on the shell. That is to say, it has the form of a half oval, the straight side of which looks dorsally and a little forward. The upper and anterior portion of the muscle is darker than the rest and sharply defined from it (Fig. 1, *add.*).

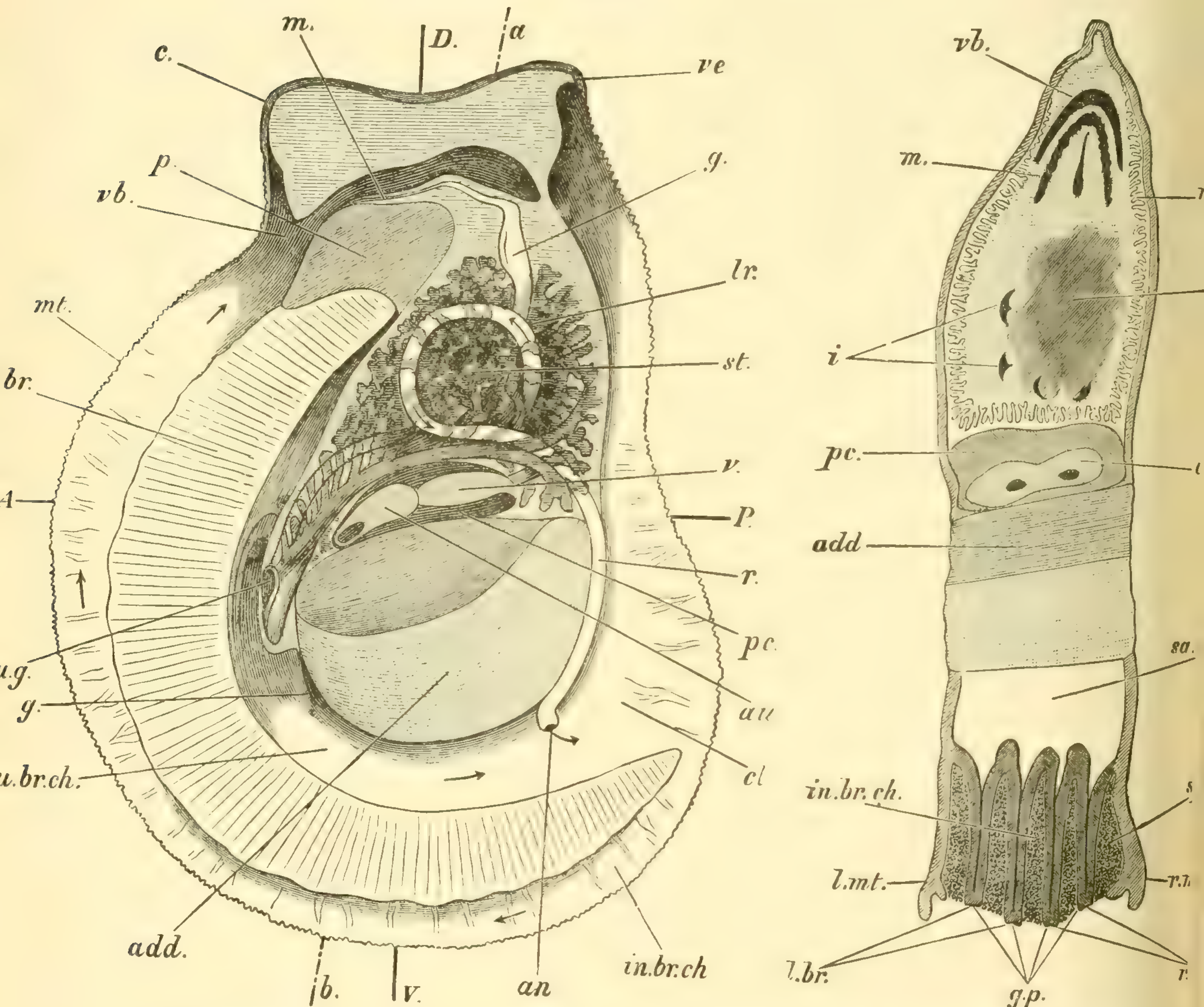
Just above the straight side of the muscle, a dark patch indicates the place of the heart, which may be seen pulsating in the chamber or *pericardium* which contains it (Fig. 1, *h*). Above this the surface of the body is covered by a smooth and delicate skin or integument, through which, in the breeding season, the reticulated whitish tubes of the reproductive gland shine.

The chief part of the body of the oyster, which for want of a better name may be termed the *trunk*, is a somewhat pyriform mass which extends from the ventral contour of the adductor to the posterior half of the dorsal region, and lies much more in the posterior than in the anterior half of the body (Fig. 2 (A)).

The rest of the body of the mollusk is chiefly formed by two broad folds of the integument which are given off from the lateral margins of the trunk on each side; extend backwards, forwards, and downwards; and, closely applied to the inner surfaces of the valves, end by thickened free margins, which have two rows, an inner and an outer, of close-set papillæ. These three folds of the integument are called the *lobes of the mantle* (Fig. 2 (B), *r.mt*, *l.mt*). Their surfaces are attached by a series of delicate muscular fibres to the inner surface of the shell, at some distance from its margin and from their own free edges; and, in the living state, the fringe, beyond the line of attachment, extends to the edges of the gape and plays the part of a sensory apparatus. The margins of the mantle lobes pass into one another above, at the anterior and posterior ends of the dorsal integument respectively, so that the cleft between them does not extend on to the dorsal surface. The lobes are much deeper in front and below than behind; hence the cavity which they inclose is correspondingly deeper and shallower in the respective regions. If one lobe is cut through, immediately beneath the anterior end of the dorsal integument, and turned back, it is seen to bound a wide space which extends back a long way, in fact nearly to the posterior side of the trunk. This is the *vestibule* (Fig. 2 (A), *vb*), and the dorsal integument which covers it is the anterior hood or *cucullus* (Fig. 2 (A), *c*). Projecting into this is seen a sort of cone which at its upper and front end bears the wide slit-like mouth, bounded by broad lips, one above and one below. The angles of these lips are produced like an upper and lower moustache, into two broad triangular flaps, the so-called *labial palps* (Fig. 2 (A), *p*). Below these, the mantle lobes, throughout their anterior and ventral regions, include a wide space termed *infra-branchial* (Fig. 2 (B), *in. br.ch.*), because the four plates which constitute the gills or *branchiæ*, and are commonly called the "beard," project into it and form its roof. Each of these plates or *lamellæ* is V-shaped in transverse section

Fig. 2 (B)). The adjacent upper ends of the four V's (VVVV) are united together, while the outer ends of the right and left V's respectively are attached to the corresponding lobes of the mantle. Hence

Fig. 2.



(A) Dissection of an Oyster from the left side. (B) Transverse section of the same taken along the line *a. b.* in A.

D. dorsal; *V.* ventral; *A.* anterior; *P.* posterior side; *mt.* mantle; *r.mt.*, *l.mt.* its right and left lobes; *c.* cucullus, or anterior hood; *ve.* velamen or posterior hood; *vb.* vestibule; *cl.* cloaca; *m.* mouth; *p.* palps; *g.* gullet; *st.* stomach; *i.* intestine; *r.* last part of the intestine; *an.* vent; *lr.* liver; *pc.* pericardium; *au.* auricle; *v.* ventricle of the heart; *br.* branchiæ; *gp.* the four lamellæ of the branchiæ, or gill plates, of which two make up the left branchia (*l.br.*), and two the right (*r.br.*); *su.br.ch.* supra-branchial chamber; *in.br.ch.* infra-branchial chamber, the position of the "spat," or mass of eggs, being shown in the transverse section; *u.g.* urogenital aperture. The duct of the left reproductive gland is seen passing from it and ramifying over the stomach and intestine. In the transverse section, the cæca of the gland are shown forming a layer immediately beneath the integument. Those of the right gland are marked *r.g.*; *g.* position of the two principal nervous ganglia; *add.* adductor muscle.

The arrows in (A) indicate the course of the inflowing and outflowing currents.

the gill plates hang down, like so many elongated Gothic sickle-shaped pendants, from the roof of the branchial cavity, which is formed by their conjoined edges. On the posterior side, the cavity inclosed between the pallial lobes is deep below, but rapidly becomes shallower above, where the lobes are narrowed to mere bands. The two bands pass into one another at the posterior end of the dorsal integument—and form a rudimentary posterior hood, the *velamen* (Fig. 1 (A), *ve*), which is very large in many other Lamellibranchs. The intestine projects beneath the integument as it runs obliquely downwards, over the posterior face of the adductor, to end in the short but prominent tubular vent. The posterior interpallial space into which this opens answers to the *cloacal chamber* of other Lamellibranchs (Fig. 1 and Fig. 2 (A), *cl.*). It is continued forwards, between the trunk and the dorsal faces of the gills, into a long *supra-branchial chamber* (Fig. 2 (A), *su.br.ch.*), which extends forwards and upwards, in front of the trunk, as far as the anterior and superior ends of the gills. For their anterior third, the dorsal edges of all the gills become attached to the front face of the trunk. The supra-branchial chamber thus becomes subdivided into four passages, which end blindly in front. The *intra-lamellar* cavities, which are inclosed by each V-shaped branchial plate, communicate, either indirectly through these passages, or directly, with the supra-branchial chamber, and this widens behind into the cloacal chamber, which opens freely on to the exterior upon the posterior side of the body. But the supra-branchial chamber, its passages, and the intra-lamellar cavities, would be completely shut off from the infra-branchial chamber by the walls of the gill plates, were it not that these walls are perforated, like a sieve, by multitudes of very narrow parallel slits.

The mouth of the oyster leads into a wide gullet, which passes back for a short distance, and then dilates into a spacious stomach, the lower and anterior end of which is continued into the long conical first part of the intestine, which passes downwards in front of the adductor, closely applied to its anterior contour. The next portion of the intestine then bends sharply upon itself and turns forwards parallel with the first part; crosses this on the right side, runs up along the back of the stomach, bends forwards and downwards, and turning back on the left side of the stomach (thus forming a completely circular loop) passes at first backwards and then downwards to the vent, the place of which has been already described (Fig. 2 (A)).

The stomach and the circular loop of the intestine are surrounded by a dark brown or greenish organ, the short branched tubules of which unite into larger tubes or ducts which open into the stomach. This organ is known as the liver—though it by no means exactly answers to the organ so called in the higher animals, but secretes the fluid which is the chief agent in digestion.

The heart (Fig. 2 (A)) lies in a spacious pericardial cavity (*pc.*)

situated between the flat face of the adductor muscle, behind and below, and the mass of the digestive viscera in front and above. It consists of a large dark-coloured auricular division (*au.*), partly divided into two, which is situated below and in front and which communicates by two short tubular passages with the pear-shaped ventricle (*v.*), the long axis of which is directed upwards and backwards. Large arterial trunks are continued from the ventricle, one upwards and backwards along the posterior moiety of the circular loop of the intestine, one forward along the anterior moiety, one downwards to the adductor. The successive contractions of the auricle and the ventricle may be readily seen in the living oyster. The blood is colourless and contains numerous colourless corpuscles. It is conveyed by the arteries to all parts of the body and thence proceeds to a large venous canal, which lies in the middle line of the anterior face of the trunk. From this it passes through the renal organs to the gills, and is thence returned by a main vessel on each side to the auricular division of the heart. The branchiæ consist of the four sickle-shaped plates already mentioned, which extend, in pairs, from the palps in front and above, to near the level of the vent behind (Fig. 2 (A) *br.*). Unlike a sickle, however, it is the convex edge of each which is sharp, while the concave edge is broader. Each plate or lamella, as we have seen, is V-shaped, consisting of two *laminæ* which bound the intra-lamellar cavity, and join below to form the edge of the lamella. It can be shown that each gill plate answers to half the gill of those Lamellibranchs in which the structure of the branchia retains its primitive simplicity. Consequently the oyster has two gills and each lamella is a *hemi-branchia* made up of two *laminæ.* Of the three partitions which separate the supra-branchial passages, the right and left represent the stems of the branchiæ, while the middle one is formed by the adherence of the edges of the inner laminæ of the two inner hemi-branchiæ to one another and to the anterior face of the trunk. Even to the naked eye the surface of a hemi-branchia appears marked with regular parallel transverse lines. And a low magnifying power shows that these lines are the optical expression of a series of parallel foldings of the lamina. The re-entering angles of the opposite folds correspond and are united together for some distance, so that the intra-lamellar chamber, or cavity of the hemi-branchia, is divided into a series of parallel transverse tubular cavities, which are widely open above, but which narrow and apparently become closed below. The lamina itself consists of close-set parallel *branchial filaments.* Each of these filaments has the shape of a lath, about $\frac{1}{1300}$th of an inch thick, and five or six times as wide; and they are set edge-wise with their flat faces not more than $\frac{1}{5000}$th of an inch apart. At intervals, transverse bands unite these lath-shaped branchial filaments together. The outwardly turned edges of the "laths" are closely beset with very long vibratile hair-like processes, known as *cilia;* and, during life, these work in such a fashion as to drive the water through the narrow clefts between the branchial filaments into the cavities of the tubes,

whence it escapes into the supra-branchial passages and chamber and thence makes its way out by the cloacal chamber. The place of the water thus swept out of the infra-branchial chamber is, of course, made good by a corresponding flow between the pallial lobes into it. Hence, while the oyster is alive, and in its proper element, a powerful stream constantly sets in on the ventral and anterior side of the body and pours out from the cloacal opening on the posterior side. The direction of the stream is marked by the arrows in Fig. 2 (A).

It is upon the proper maintenance of this current that the life of the oyster depends. For these animals feed upon the microscopic organisms, largely consisting of diatomaceous plants, which live in the sea; and as they possess no organs for seizing such food, they are almost entirely dependent for their supply of nourishment upon the indraught caused by the cilia on the gills, and especially upon those which line the edges of the branchial plates and direct a portion of the current towards the mouth. The anterior ends of each pair of hemibranchiæ are attached between the two palps of the side to which they belong. The applied surfaces of the palps, between which lies the commencement of the mouth-cleft, are ridged and richly ciliated, so that anything brought by the ciliary current of the gills is led directly into the oral cavity. The cilia which line this eventually drive it into the stomach. Thus the unimpeded action of the cilia of the gills is essential to the nutrition of the oyster; but it is not less necessary to its respiration, to the carrying away of the waste products of the renal and alimentary organs, and to the expulsion of the reproductive products. For all these processes depend, either on the flow of water through the laminæ of the gills, or upon the current which sets out from the supra-branchial and cloacal chambers.

Hence the importance of tolerably clear water to oysters. If turbid water, laden with coarse sediment, enters the infra-branchial cavity, particles of mud, too large to be moved by the cilia, lodge upon the gills, and, gradually obstructing the current, interfere with the primary functions of feeding and breathing to such an extent as to injure, or even to destroy the animal.

It would be out of place here to give any account of the complicated renal organs of the oyster, recently discovered and described by Dr. Hoek. But it is necessary to notice the openings by which the cavity common to them and the reproductive organs debouches. These are the small slit-like apertures (Fig. 2 (A), *ug.*) situated one on each side of the lower and front face of the trunk, which open into the supra-branchial cavity.

The nervous system of the oysters is more poorly developed than that of any of their allies among the lamellibranchiate mollusks. Only two out of the three pairs of nerve masses, or *ganglia*, which these animals ordinarily possess have been clearly made out, while, of these, the pair which is most likely to represent the sensorium of higher animals is exceedingly small. Moreover, no organs of special sense have been demonstrated. So that, if any reasoning from

analogy is permissible on this subject, it is probable that the sensibility of the oyster is infinitesimally small.

The oyster, as we have seen, possesses one very large adductor muscle, but only one. Almost all other Lamellibranchs (*e. g.* cockles, mussels, razor-fish) have two ; one in front, near the mouth ; and one behind, in a position which exactly answers to that of the single adductor of the oyster. The latter, therefore, is called *monomyary*, or one-muscled, while the former are *dimyary*, or two-muscled ; and a series of forms can be selected among the sea mussels and the scallops which show the posterior adductor becoming larger and larger, while the anterior diminishes, until, in the oyster, it disappears.

During the summer and autumn months, from as early as May to as late as, or even later than, September, according to circumstances, of which the temperature and the depth of the water in which the oysters live appear to be the most influential, a certain proportion of the oysters in an oyster-bed pass into a peculiar condition, and are said by the fishermen to be "sick." In about half of these sick oysters, a whitish substance made up of innumerable very minute granules, imbedded in, and held together by, a sort of slime, collects in the infra-branchial chamber, filling up the interspaces between the mouth and the gills, and between the gill plates themselves, and even occupying the vestibular cavity so completely that it is difficult to understand how the processes of breathing and feeding can be carried on (Fig. 2 (B)).

This granular slime is what is known as "white spat," and the granules are the eggs of the oyster. By degrees, the granules become more or less coloured; and the mass, acquiring a brownish hue, is termed "black spat." This change depends on the development of the young, which acquire a certain degree of coloration within the eggs. At the end of a period, the length of which varies with the temperature of the water and other conditions, but appears rarely to exceed a fortnight, the mass of black spat breaks up, and the young, hatched out of the eggs, leave the mantle cavity of the parent in which they have been thus incubated. They become diffused through the water, and swarm in vast multitudes at the surface of the sea.

A single full-grown oyster produces, on the average, about a million of these free-swimming young or *larvæ*. If a glass vessel is filled from the stratum of surface-water in which the larvæ swim, and held up to the light, it will appear full of minute particles—only $\frac{1}{150}$th of an inch long, and therefore just visible to the naked eye—which are in active motion. An ordinary hand-magnifier is sufficient to show that these minute organisms have very much the aspect of the *Rotifera*, or "wheel animalcules," so common in fresh water. They have a glassy transparency, and are colourless, except for one or more dark brown patches; while, at one end, there is a disk, like the "wheel" of the Rotifers, the margins of which are apparently in rapid motion, and which serve as the organs of pro-

pulsion. When this propeller is moderately active, the larvæ dance up and down in the water, with the disk uppermost; but when the action is more rapid, they swim horizontally with the disk forward.

How long the larval oysters remain in this locomotive state, under, natural conditions, is unknown, but they may certainly retain their activity for a week, as I have kept them myself in a bottle of sea-water, which was neither changed nor aerated, for that period. But, sooner or later, they settle down, fix themselves by one side to any solid body, and rapidly take on the characters of minute oysters, which have the appearance of flattened disks, $\frac{1}{20}$th of an inch, more or less, in diameter; they are therefore perfectly visible, as white dots, on the surface of the substance to which they adhere. In this condition, the name of "spat" is also applied to them. The locomotive larvæ being practically invisible in the sea, this spat appears to be as it were precipitated out of the water; and, since great quantities appear at once, the oyster fishermen speak of a "fall" of spat.

It is important to observe, that when oyster fishermen say that there has been no "fall" of spat in a given season, all that is really implied is that the young fixed oysters have not made their appearance. The fact of the absence of a "fall of spat" does not justify the conclusion that the oysters have not bred as usual. It is quite possible, that just as many eggs have been deposited in the branchial cavity, and that just as many larvæ have been set free as in other years; but that the larvæ have been destroyed by those changes of temperature to which they are so sensitive, or by other causes. But, of course, it is also quite possible that the oysters have been really barren; or that, although the eggs have reached the mantle cavity, the larvæ have not hatched out. Oyster eggs, no less than hens' eggs, may be addled.

It is obviously useless to speculate upon the causes of a "failure of spat," until, by the examination of samples of oysters from time to time, and by sweeping the superjacent water with a fine towing-net, the exact nature of the particular case of failure has been ascertained. There is much reason to believe that the fertility of oysters preserved in parks is greatly diminished, although the oysters themselves may be improved in fatness and quality by the process, and that this is especially the case when the water in which they are preserved has a low degree of salinity; and it is very desirable to ascertain the nature of the modifications effected in the structure and functions of the reproductive apparatus of the oyster under these circumstances.

It is unfortunate that the same word "spat" should be applied to things so different in their nature, as the eggs and unhatched young of the oyster contained within the mantle cavity on the one hand, and the young fixed oysters on the other; while there is no familiar name for the very important stage of development which lies between these two. "Brood," "fry," and "spat" would be very convenient names for the three stages, if "brood" were not already in use for the

smallest of the young fixed oysters. Perhaps the most convenient course will be to use "fry" for the eggs or embryos which are contained within the mantle cavity of the parent; "larvæ" for the locomotive stage; and "spat" for the final condition.

In order to become spat, the larva appears invariably to fix itself by one side (almost always the left); and, if the surface is favourable, the extent of the surface of adhesion becomes very considerable, and the oyster is fixed throughout life. But, if the surface of adhesion is small, the oyster, as it increases in size, readily becomes detached and lies free, though motionless, on the bottom.

The young oysters grow very rapidly. In five or six months, they attain the size of a threepenny piece; and, by the time they are a twelvemonth old, they may reach an inch or more in diameter. The rate of growth varies with the breed of oyster, and with the conditions to which it is exposed; but it is a roughly accurate and convenient way of putting the matter to say, that, at two years, the oyster measures two inches across, and at three years, three inches. After this, which may be regarded as the adult age, the growth is much slower, and the shell increases in thickness, much more than in circumference.

The natural term of the oyster's life is not known, but there is reason to believe that it may extend to twenty years or more. An excellent authority, Professor Möbius, is of opinion that most of the adult Schleswig oysters are from seven to ten years old, and that, though oysters over twenty years of age are rare, he has met with occasional specimens which had attained between twenty-five and thirty years.

Oysters breed long before they are full grown, very probably in the first year of their age, certainly in the second. Their productivity appears to reach its maximum at five or six years, and afterwards to decline; but much further observation is needed before any definite rules can be laid down on this subject.

These are the most important obvious phenomena presented by the reproductive processes of the oyster.* We must now consider them a little more in detail, and under those aspects which are hidden from ordinary observation.

The oyster, like other animals, takes its origin in an egg, or *ovum*, a minute, relatively structureless, protoplasmic spheroidal body, about $\frac{1}{250}$th of an inch in diameter, by a long series of developmental changes which take place in that ovum after it has united with another living particle of extremely minute size, the *spermatozoon*, and in consequence of the fertilisation effected by that union, just as the ovule of a plant develops in consequence of the influence of the pollen

* It must be remembered that the account here given holds good only of the *Ostrea edulis* of England and Northern Europe. In the Portuguese oyster (*O. angulata*) and the American oyster (*O. virginiana*) the eggs are set free at once, and are not incubated in the mantle cavity of the parents.

upon it. And the first problem is, Where are these ova and spermatozoa formed? Does each oyster produce both, or are they formed in distinct oysters? This is, in fact, the vexed question of the sexes of the oyster, which has been the subject of so much discussion, and for which the answer is gradually shaping itself, thanks mainly to the recent labours of Möbius and Hoek.

I have already stated that if the surface of the trunk of a full-grown oyster is examined carefully with a lens, or even without one, a curious ramified and more or less reticulated whitish marking, which is very obvious in the breeding season, is observable beneath the thin integument. By appropriate methods of investigation it is easily determined that this marking is produced by the ramifications of a tubular organ,—*the reproductive gland*—the trunk of which debouches into a cavity common to it and the renal organs, which again, it will be recollected, communicates by a narrow slit with the supra-branchial chamber (Fig. 2 (A), *ug*). The trunk of the gland, on each side, passes upwards and backwards, in front of and above the adductor and muscle, and gives off a multitude of branches, some of which cross the middle line and become inextricably united with those of the other side, while others form a network beneath the skin which covers the stomach and the liver. From this network, blind offshoots are given off perpendicularly inwards, and extend for a variable depth into the interior of the body. The whole extent of the walls of the tubes of this reproductive gland is lined by nucleated cells, and it is by the metamorphoses of these cells that the ova, on the one hand, and the spermatozoa, on the other, are produced.

During the breeding season, an examination of the adult oysters on an oyster-bed shows that the number of individuals the reproductive glands of which contain hardly anything but ova is about equal to that of the individuals in which the reproductive gland contains hardly anything but spermatozoa. I say "hardly anything" because competent observers have affirmed, that careful search will always reveal a few spermatozoa in the former, and a few ova in the latter. Whether this be so or not, there can be no doubt that, practically, oysters, while actually breeding, are either males or females.

When the ova or spermatozoa are ripe, they flow out of the reproductive gland into the surrounding water. The spermatozoa are carried away by the exhalent currents of the oyster in which they are developed, and are doubtless drawn in by the inhalent currents of adjacent oysters, the eggs of which they fertilise. And, as the eggs already exhibit the first of that series of changes which lead to the formation of the larva, when they leave the reproductive gland, it would appear that they must undergo fertilisation while still within that organ.

The eggs which pass into the supra-branchial chamber must also be driven out by the exhalent current; but it would seem that, when they reach the hinder edge of the branchial partition, they come

within the influence of the inhalent current and are thereby swept back into the infra-branchial chamber. Here they accumulate, and becoming imbedded in a viscid albuminous matter, secreted by the parent, constitute the "white" fry (Fig. 2 (B)).

From the nature of the case, this account of what takes place is not the result of direct observation; but it seems to be by far the most probable explanation of the facts which can be observed. In an oyster which contains white fry, in fact, the reproductive gland is flaccid, and contains nothing, or hardly anything, but a few unexpelled ova.

The case is different, however, with oysters the eggs of which have been laid so long that they have passed into the condition of "black spat." Here many, or, as I have recently found in one case, the great majority, of the tubes of the gland contain developing spermatozoa, while only a few exhibit ova. And Dr. Hoek has recently made the important observation that, if an oyster which contains fry is kept for a fortnight in an aquarium by itself and then examined, the reproductive organ will be found no longer to contain ova, but abundant developing and fully formed spermatozoa.

After producing eggs, in fact, the female oyster changes its sex and becomes male.

The conclusion, first advocated by M. Davaine many years ago, that the same individual oyster is alternately male and female, is therefore, unquestionably correct. What has yet to be made out is the period of recurrence of this extraordinary alternation of sexes. Do oysters change their sexes once or more than once in a season? Until this point is ascertained, all calculations as to the proportionate number of oysters which breed during a season, based on the observation of the proportion of those which at any given time contain fry, are obviously unsafe. If, for example, the alternation took place once a month, not more than half the oysters might at any time contain fry, and yet, in four months, every oyster might have spatted twice.

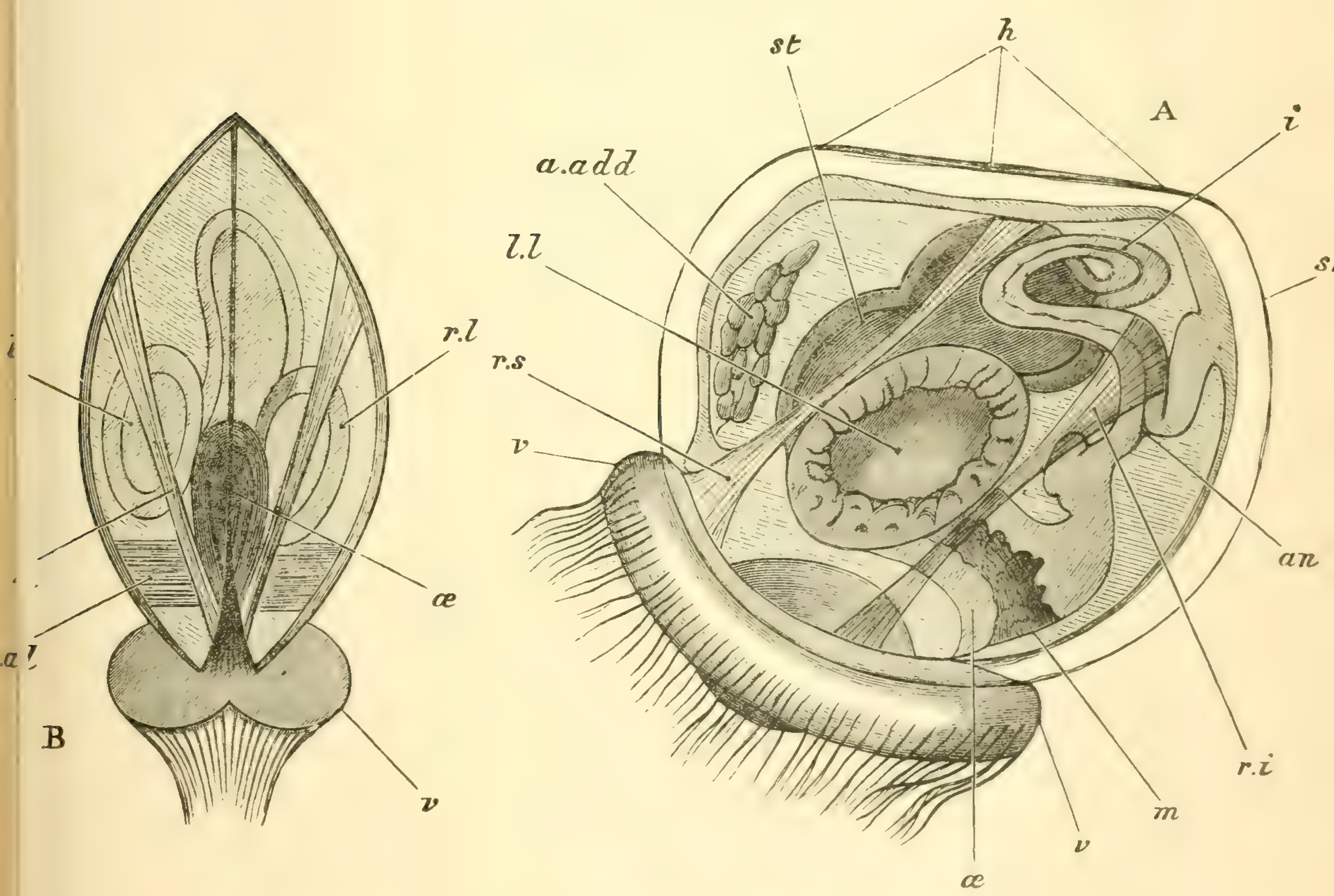
In the case of the Portuguese and the American oysters, in which both the reproductive products pass at once into the water and no incubation takes place, artificial fecundation is easily effected. The embryos develop normally, pass through their changes within the egg, and their locomotive stage, into the condition of fixed oysters rapidly, when confined in properly arranged aquaria. It is probable, therefore, that artificial breeding will sooner or later be practised on a great scale with these oysters. In the case of our own oysters, artificial propagation by the methods practised in the case of the Portuguese and American forms, which involve the destruction of both parents, is obviously out of the question, unless some substitute can be found for the process of incubation, during which it is probable that the young oysters receive, not merely shelter but nourishment, from the parent. But a careful study of the conditions under which our oysters breed freely, will no doubt enable oyster cultivators to

imitate these conditions—and to place their breeding stock under circumstances in which hurtful influences shall be excluded, while the larvæ are prevented from wandering too far and facilities are afforded for their attachment.

It has been seen that the young animal which is hatched out of the egg of the oyster is extremely unlike the adult, and it will be worth while to consider its character more closely than we have hitherto done.

Under a tolerably high magnifying power the body is observed to be inclosed in a transparent but rather thick shell, composed, as in the parent, of two valves united by a straight hinge (Fig. 3 A. *h*). But these valves are symmetrical and similar in size and shape, so that

Fig. 3.



The larva of the Oyster.

A, side view. B, front view. *v.* velum with its long cilia; *æ.* œsophagus or gullet; *st.* stomach; *r.l.*, *l.l.* right and left lobes of the "liver"; *i.* intestine; *an.* vent; *a. add.* anterior adductor muscle which alone exists in the larva; *r.s.*, *r.i.* superior and inferior muscles which retract the velum into the shell, *sh.*; *h.* hinge of the shell.

the shell resembles that of a cockle more than it does that of an adult oyster. In the adult the shell is composed of two substances

of different character, the outer brownish, with a friable prismatic structure, the inner dense and nacreous. In the larva, there is no such distinction, and the whole shell consists of a glassy substance devoid of any definite structure.

The hinge line answers, as in the adult, to the dorsal side of the body. On the opposite, or ventral side, the wide mouth (*m.*) and the minute vent (*an.*) are seen at no great distance from one another. Projecting from the front part of the aperture of the shell there is a sort of outgrowth of the integument of what we may call the back of the neck, into a large oval thick-rimmed disk termed the *velum* (*v.*), the middle of which presents a more or less marked convex prominence. The rim of the disk is lined with long vibratile cilia, and it is the lashing of these cilia which propels the animal, and, in the absence of gills, probably subserves respiration. The funnel-shaped mouth has no palps; it leads into a wide gullet and this into a capacious stomach. A sac-like process of the stomach on each side (*rl.*, *l.l.*) represents the "liver." The narrow intestine is already partially coiled on itself, and this is the only departure from perfect bilateral symmetry in the whole body of the animal. The alimentary canal is lined throughout with ciliated cells, and the vibration of these cilia is the means by which the minute bodies which serve the larva for food are drawn into the digestive cavity.

There are two pairs of delicate longitudinal muscles (*r.s.*, *r.i.*) which are competent to draw back the ciliated velum into the cavity of the shell, when the animal at once sinks. The complete closure of the valves is effected, as in the adult, by an adductor muscle, the fibres of which pass from one valve to the other (Fig. 3, *a. add.*). But it is a very curious circumstance that this adductor muscle is not the same as that which exists in the adult. It lies, in fact, in the fore part of the body, and on the dorsal side of the alimentary canal. The great muscle of the adult, on the other hand, lies on the ventral side of the alimentary canal and in the hinder part of the body. And as the muscles, respectively, lie on opposite sides of the alimentary canal, that of the adult cannot be that of the larva which has merely shifted its position; for, in order to get from one side of the alimentary canal to the other, it must needs cut through that organ. But, as in the adult, no adductor muscle is discoverable in the position occupied by that of the larva, or anywhere on the dorsal side of the alimentary canal; while, on the other hand, there is no trace of any adductor on the ventral side, in the larva—it follows that the dorsal or anterior adductor of the larva must vanish in the course of development, and that a new ventral or posterior adductor must be developed to play the same part and replace the original muscle functionally, though not morphologically.*

* The larva of the cockle has, at first, like the oyster larva, only one adductor, which answers to the anterior of the two adductors which the cockle possesses in the adult state.

This substitution is the more interesting since it tends to the same conclusion as that towards which all the special peculiarities of the oyster lead us; namely, that, so far from being a low or primitive form of the group of lamellibranchiate mollusks to which it belongs, it is in reality the extreme term of one of the two lines of modification which are observable in that group. The *Trigoniœ*, the arks, the cockles, the freshwater mussels, and their allies, constitute the central and typical group of these mollusks. They possess two sub-equal adductors, a large foot, and a body which is neither very deep nor very long. From these, the series of the boring bivalves exhibits a gradual elongation of the body, ending in the ship-worm (*Teredo*) as its extreme term. While, on the other hand, in the sea mussels, the *Aviculœ*, and the scallops, we have a series of forms which, by the constant shortening of the length and increase of the depth of the body, the reduction of the foot, the diminution of the anterior of the two adductors, and the increase of the posterior, until the latter becomes very large and the former disappears, end in the oyster.

And this conclusion, that the oysters are highly specialised Lamellibranchs, agrees very well with what is known of the geological history of this group, the oldest known forms of which are all dimyary, while the monomyary oysters appear only later.

When the free larva of the oyster settles down into the fixed state, the left lobe of the mantle stretches beyond its valve and applying itself to the surface of the stone or shell, to which the valve is to adhere, secretes shelly matter, which serves to cement the valve to its support. As the animal grows, the mantle deposits new layers of shell over its whole surface, so that the larval shell valves become separated from the mantle by the new layers which crop out beyond their margins and acquire the characteristic prismatic and nacreous structure. The summits of the outer faces of the umbones thus correspond with the places of the larval valves, which soon cease to be discernible. After a time, the body becomes convex on the left side and flat on the right; the successively added new layers of shell mould themselves upon it; and the animal acquires the asymmetry characteristic of the adult.

Oysters are gregarious, in consequence of the vast multitude of locomotive larvæ which are set free simultaneously ; and which, being subjected to the same influences, tend to settle about the same time in the area to which the swarm drifts. Millions of oysters are thus aggregated together over stretches of the bottom of the sea, at depths of from one or two, to twenty or more, fathoms, and constitute what are known as oyster beds.

Although oysters live and grow well enough in estuaries, in which the salinity of the water undergoes large variations, according to the state of the tide and the volume of fresh water that is poured in, yet they do not flourish permanently and breed freely in water with less than 3 per cent. of saline constituents. Thus the Baltic is, at pre-

sent, unfit for their support; and the east coast of Schleswig, washed by its brackish waters, is devoid of oysters, while certain parts of the west coast are famous for their oyster beds. Gravel, stones, and dead shells—commonly known as "cultch"—form the most favourable bottom, as they facilitate the attachment of the young. Disturbed muddy bottoms, on the other hand, are fatal, for reasons which have already been given. But it is a curious fact, that even where a large extent of sea-bottom presents apparently the same conditions, oyster beds occur in some localities and not in others.

The struggle for existence is as intense in the case of the passive oyster as in that of the most active of animals. Oyster competes against oyster for the common store of food suspended in the water, and for the dissolved carbonate of lime out of which the shell must be made. Innumerable other animals, sponges, corallines, polypes, tunicates, other bivalve mollusks, especially mussels and cockles, live in the same way and abound on oyster beds, often attached to the shells of the oysters. Prof. Möbius counted as many as 221 distinct animals of various species on one oyster shell. All these compete with the oyster for food, while, on the other hand, they may occasionally supply food to the oyster in the shape of debris, and, perhaps, of their eggs and microscopic larvæ.

From birth onwards oysters are the prey of many animals. The minute larvæ, as they swim about, are probably swallowed by everything which has a mouth large enough to admit them; and, as soon as the young oysters have become sedentary, they are eaten by everything which has jaws strong enough to crush them. Ground fishes, such as rays and fish of the cod tribe, easily break them up when they have grown much larger; while starfishes swallow them whole. Even the half-grown oyster, with a shell strong enough to resist most teeth, and too big for the maw of an ordinary starfish, is not safe from the depredations of the dogwhelk (*Purpura lapillus*) and the whelk tingle (*Murex erinaceus*), which effect a burglarious entrance, by means of the centre-bit with which nature has provided their mouths. It is very curious to watch a dogwhelk perched upon an oyster shell and patiently working, hour after hour, until the little and apparently insignificant tunnel, by which the insidious enemy will get access to the fat prey within, is completed. If you pull him off, he puts on as soft a look as the most innocent snail could do, as who should say, "Why prevent me from establishing closer intercourse with the dear neighbour at the other end of my tunnel?" The guardians of the oyster, however, who have not much of the "friend of humanity" about them, ruthlessly arrest the operations of the tunnellers by sudden squash with boot or hammer. And well they may, for they have few more dangerous adversaries. In the Bay of Arcachon, 14,000 whelk tingles were picked off 100 acres of oyster ground in the course of a month.

Other animals injure the oyster indirectly by mining in the shell. The boring sponge, *Cliona*, does this; and a very curious instance of

mischief done in this way by a burrowing annelid (*Leucodore*) was recently brought to my notice by Sir Henry Thompson. The *Leucodore* drives burrows into the shell and lives in them without any evil intent towards the oyster. But the burrows fill with fine mud, and this, spreading into the vacuities of the shell, gives rise to inky patches, which look unpleasant when the oyster is opened and damage its commercial value, though, as I can testify, the flavour of the oyster is nowise impaired.

The larval oysters are extremely sensitive to cold, and any sudden fall in the temperature of the air during the swarming time is fatal to them. Even the adult oyster is readily killed by sudden frosts, if the water is sufficiently shallow to allow the change of temperature to penetrate. Great heat is equally pernicious. At Arcachon, immense numbers of oysters were killed by the hot summer of 1870.

To this long list of influences against which every oyster has to struggle successfully, if it is to attain maturity, larger knowledge will doubtless add many others. But these are enough to enable us to understand why it is that the increase of a given stock of oysters may be, and usually is, very slight, notwithstanding the prodigious fertility of the individual oyster. A very large proportion of the oysters in a bed, under ordinary circumstances, breed during the season; and, as each adult female oyster, on an average, gives rise to a million eggs, one would expect a prodigious increase, even if nine-tenths of the young were destroyed. But from the small proportion of half-grown to full-grown oysters (40–50 per cent.) it is clear that the real addition to the oyster population, in most years, is very small. It is probable, in fact, that unless the conditions are unusually favourable, not more than two or three out of every million of the fry of the oyster ever reach maturity.

It is obvious that the conditions of existence of the oyster are of an extremely complicated character, and that the population of an oyster bed, under natural conditions, must be subject to great fluctuations. A few good spatting years, accompanied by a falling off in the number of starfishes and dogwhelks, may increase it marvellously, while the contrary conditions may as strikingly reduce it.

Man interferes with this state of things in two ways. On the one hand he is one of the most efficient of destroyers, and on the other, he is the only conservator of the mollusks, albeit his conservation is with a view to ultimate destruction. Let us consider him first under the aspect of destroyer. In some places, oysters are taken at low tide by the hand; but usually they are captured by means of the dredge, which is essentially a bag, the sides of the mouth of which are fashioned into scrapers. The dredge is drawn slowly over the oyster bed for a certain time, and the oysters, with multitudes of other animals, stones, and the like, are scraped into the bag. This is then hauled up, and the contents emptied on to the deck; the oysters are picked out and the refuse returned to the sea.

There can be no doubt that the great mass of oysters in an oyster bed may be removed by systematic and continuous dredging. But those who are best acquainted practically with the nature of that operation will be least inclined to believe that all the oysters on a bed could be cleared off in this way, even if the attempt were made; and, as it must cease to be profitable to dredge long before the point of entire clearance is reached, it is plain that, in practice, the attempt will not be made. It may be doubted if ordinary dredging ever fails to leave some thousands of oysters, great and small, on a bed of any extent.

Thus, if we admit, for the sake of argument, that an oyster bed may be exhausted by ordinary dredging, the reason why the oysters vanish is not obvious. For, supposing only a thousand oysters left, they ought to suffice to restore the bed by degrees. I am aware that it is said that, in the meanwhile, the enemies and competitors of the oyster have got the upper hand, that the ground has been spoiled by accumulation of mud and so on. But this reasoning leaves out of sight the fact that the oysters have not been there from all eternity. There was a time when there were no oysters on the ground, and when the oyster larvæ immigrated, they fixed themselves there, increased and multiplied, in spite of all obstacles. Why should they not do so again?

The question is further complicated by the consideration that it is by no means certain whether the population of a given oyster bed is kept up by the progeny of its own oysters or by immigrants. As I have pointed out, it is ascertained that the larvæ, even under very unfavourable circumstances, may swim about for a week; and it has been estimated that they are ordinarily locomotive for two or three times that period. Even if we suppose the average period of freedom to be not more than three days, the chance that an oyster larva will eventually settle within a mile of the spot at which it was hatched, in any estuary or in the open sea, must be very small. For, in an estuary, and almost always in the sea, one of the two alternating currents of water is dominant, and a floating body will drift, on the whole, in that direction, often many miles in the course of a day.

The opportunity of observing the natural formation of a new oyster bed is rare, but the details of the process have been carefully watched in at least one case. Up to the year 1825, the Limfjord in northern Jutland consisted of a series of brackish water lakes communicating with one another, and opening on the east into the Kattegat. In the last century, unsuccessful attempts were made to plant them with oysters. But, on the 3rd of February, 1825, a great storm broke through the dam which separated the western part of the Limfjord from the North Sea; in consequence of this, the water of the fjord became gradually salter, the brackish water plants and animals disappeared and North Sea animals took their places. Among these, in 1851, oysters were observed, and, year by year, they extended over a larger area. In 1860, only 150,000 were taken; at present,

there are ninety-eight beds, and, in 1871–1872, 7,000,000 of full-grown oysters were exported. There could have been very few oysters before 1851, when the first were noticed. But supposing the first entered as early as 1840, then, in thirty years, they spread themselves over an area of about sixty-four English miles, so that every year, on the average, they advanced more than two miles. The oyster-beds are, at present, three-fifths of a mile to five miles apart, so that the larvæ must have been able to wander for at least five miles.*

During this slow process of immigration, it is obvious that the enemies and the competitors of the oysters had just as good a chance as the oysters themselves; and yet the latter have established themselves with great success. Why should they be unable to do the like elsewhere?

I must confess myself unable to arrive at a conclusion on the question whether what is called "over-dredging"—that is, dredging to the extreme limit at which it is commercially profitable to dredge—is alone competent permanently to destroy an oyster bed or not. That oyster beds have disappeared after they have been much dredged, I do not doubt. But the commonest of all fallacies is the confusion of *post hoc* with *propter hoc*; and I have yet to meet with a case in which it is proved by satisfactory evidence, that an oyster bed has been permanently annihilated by dredging, when the spatting seasons have been good, and when there has been no reason to suspect an inroad of destructive mollusks or starfishes.

Man intervenes in favour of the oyster by the process which is known as "oyster-culture." This consists in collecting the spat as soon as it has attached itself, and removing it to conveniently-situated natural and artificial shallows, known as "oyster-parks," where it can be protected from its enemies and at the same time nourished.

Practised at Whitstable and elsewhere from time immemorial, this process has more recently been developed through laying down fascines of twigs, or tiles, in the way of the oyster larvæ during the spatting season. In good spatting years, the quantity of young oysters obtained in this way is prodigious. In 1865, Mr. Nichols, the foreman of the Whitstable Company, told the Sea Fisheries' Commissioners, that, in the year 1858, the spat was very abundant, and that the brood gathered in that and the three following years formed the stock from which the market had ever since been supplied. But he added, that they did not expect a good spatting season more than once in every six years; and that, within his recollection, there had been no spat upon the flats, where it is usually collected for a period of thirteen consecutive years.

It will be observed that oyster culture is not oyster breeding, but simply a means of profiting by the abundant produce of those years

* Möbius, 'Die Auster und die Austern-wirthschaft,' p. 52.

in which the young successfully reach their fixed stage. The supply is therefore very precarious. Moreover, it is by no means easy to find localities, suited for oyster-parks, which must be protected from storms, and yet have free access to the sea; shallow, and yet not liable to become too hot in summer or too cold in winter; open to currents which bring nutriment, and yet not liable to be silted up by mud. Even when all these conditions are fulfilled, much labour and watchfulness are needed to keep the beds clean and free from the incursions of enemies. And, when all that skill and industry can do is done, ostreiculture is attended with no less risk and uncertainty than agriculture in a variable climate. Favoured by one or two fortunate spatting years, M. Coste made ostreiculture the fashion a quarter of a century ago. A large capital was embarked, in France and in this country, in establishing oyster-parks, but it may be questioned whether more than a small fraction of the investment has ever found its way back into the pockets of the investors; and, in many cases, the results have been disastrous.

The increasing scarcity and dearness of oysters were subjects of complaint twenty years ago, and the outcry has become louder of late years. Three causes, and only three, so far as I know, have been assigned for this unsatisfactory state of things: first, the increase in the demand for oysters, owing in large measure to modern facilities of transport, consequent upon the vast development of the means of locomotion; second, an unusual succession of bad spatting years; third, over-dredging, that is to say, the removal of so many oysters from the oyster beds that the number left is insufficient to keep up the stock.

That the first and the second of these causes have had a great deal to do with the matter is beyond doubt; but, whether any harm has resulted from simple over-dredging is a question respecting which very different opinions are entertained, and I have already stated my reasons for reserving my opinion on the subject. But I shall suppose, for argument's sake, that all three influences are in operation, and proceed to ask what can be done by legislation to mitigate their evil effects.

A sumptuary law restricting the consumption of oysters, per head, is not practicable in these days; and therefore, the first cause of dearness, great demand, must be left to cure itself by the increase of price to which it gives rise.

Nor is the second cause of scarcity within reach of legislation. The seasons cannot be rendered favourable to oyster spatting by Act of Parliament.

But it is very generally believed that the enforcement of what is called a "close time" is an effectual remedy for over-dredging. Oyster "close time" means that oysters shall not be taken during the months of May, June, July, and August, which are supposed, not quite accurately, to cover the breeding season of the shell-fish.

But surely nothing is more obvious than this, that the prohibition of taking the oysters from an oyster bed during four months of the year is not the slightest security against its being stripped clean (if such a thing be possible) during the other eight months. Suppose, that in a country infested by wolves, you have a flock of sheep, keeping the wolves off during the lambing season will not afford much protection if you withdraw shepherd and dogs during the rest of the year.

These considerations are so obvious, that I cannot but think that the cry for close time for oysters must be based on a confused notion that, as close time is good for salmon, so it must be good for oysters. But there is really no analogy between the two things which here pass under the name of "close time" Close time for oysters is merely protection of oysters during the breeding season; close time for salmon is not merely protection of salmon during the breeding season, it means a practical limitation of the capture of salmon all the year round by the weekly close time, supplemented by the license duties on rods and nets. You might protect the breeding grounds of salmon as strictly as you pleased and as long as you pleased; but, if too many of the ascending fish were captured, the stock would fall off, and if all were captured, it would come to an end.

If the protection afforded to an oyster bed is to be made equivalent to that given to a salmon river, measures must be taken by which the undue diminution of the stock of oysters, at any time, may be prevented. The most effectual way of doing this is to form an estimate of the number of oysters on a bed before the commencement of the open season; and to permit the removal of only such a percentage as will leave a sufficient stock. And regulations of this nature have long been carried out in the Schleswig oyster fisheries and in those of France. A subsidiary regulation, tending towards the same end, is that which enforces the throwing back into the sea of all half-grown oysters. As oysters produce young before they are half-grown, this procedure must contribute to the breeding stock.

When, nearly twenty years ago, my colleagues, Sir James Caird, Mr. Shaw-Lefevre, and I, had to deal with the oyster question, I am not aware that any of us doubted the value of protection of public oyster beds in the open sea, if it could only be made efficient.

What we are quite clear about, however, was:—

(1) That the close time regulation which then existed was always useless, and sometimes mischievous.

(2) That the regulation prohibiting the taking of half-grown oysters interfered with the transfer of oysters from the public beds, where they were exposed to all sorts of dangers, to the private grounds where they were protected.

(3) That it was practically impossible to establish an efficient system of protection on our public oyster beds.

And therefore we came to the conclusion that the best course that

could be adopted was to abolish all the delusive and vexatious regulations which were in force; and to see what could be done by giving such rights of property in parts of our shores favourable to oyster culture, as would encourage competent persons to invest their money in that undertaking.

[In the latter part of the discourse Professor Huxley commented on the results derived from Reports on the Oyster Fisheries, especially in the Bay of Cancale and the Bay of Arcachon, pointing out the uncertainty which has attended oyster culture and the inefficiency of such restrictive legislation as has hitherto been adopted.]

I for my part believe that the only hope for the oyster consumer lies first in oyster culture, and secondly, in discovering a means of breeding oysters under such conditions that the spat shall be safely deposited. And I have no doubt that when those who undertake the business are provided with a proper knowledge of the conditions under which they have to work, both these objects will be attained.

[T. H. H.]

WEEKLY EVENING MEETING,

Friday, May 18, 1883.

RIGHT HON. THE LORD CLAUD HAMILTON, J.P. Manager, in the Chair.

Professor C. E. TURNER, *of the University of St. Petersburg.*

Kustàrnoe Proiezvódstvo; *or, the Peculiar System of Domestic Industry in the Villages of Russia.*

IN the earliest stages of civilisation the family forms the social unit, as in modern times the basis of our social organisation is the individual. The elder type has been much longer maintained in Russia than elsewhere, and most strikingly presents itself in what the Russians term *Kustàrnoe proiezvódstvo.* The root of the term is to be found in the word *Kust* a bush, and is applied to the members of a family who, when united in some common labour, form as it were an organic inseparable whole and may be compared with a bush. Among the Russian peasantry there exist two kinds of families, large and small. The latter are composed of a husband, wife, and unmarried children; but the former include the married sons and daughters who still continue to live with their children in the house of their father. It is amongst the large families that Kustàrnoe industry chiefly obtains. In the division of labour, as well as in sharing the profits, a principle of equality, without any difference being made as to age or sex, is observed. The greatest care is taken to confine to each household the particular branch of trade with which it is occupied. The relations existing between the members of a family are necessarily closer and of a less irksome character than can exist between an employer and an apprentice: and in the Moscow Exhibition of 1862, we had numerous examples of the healthy influence exercised by this system on the industry of the country. Though very hard pressed by its unequal concurrence with factory labour, it still very largely prevails in the more important governments of Russia. The number of workmen employed in the factories of Russia, excluding those in Poland and Finland, does not exceed 711,000, whilst many of the branches of *Kustàrnoe* industry have taken their rise within a very recent period, and those of an earlier date produce twice or three times as much as they did before the commencement of the present century. It must for a very long time play an important part in the economical life of the people, and for this reason its development and extension should be encouraged. For this purpose, the lots of peasant land, which are inadequate to supply the elementary wants of life, should be enlarged; elementary technical schools should be opened in those districts where *Kustàrnoe* industry is greatly practised; greater encouragement should be given to the formation of co-operative associations, or *artels,* among the peasant classes; and credit-banks might also with advantage be opened by the government, and the peasant be thus enabled to secure small loans on fair and equitable conditions.

[C. E. T.]

WEEKLY EVENING MEETING,

Friday, May 25, 1883.

William Spottiswoode, Esq. M.A. D.C.L. Pres. R.S.
Vice-President in the Chair.

Professor W. H. Flower, LL.D. F.R.S. P.Z.S. &c.

On Whales, Past and Present, and their Probable Origin.

Few natural groups present so many remarkable, very obvious, and easily appreciated illustrations of several of the most important general laws which appear to have determined the structure of animal bodies, as those selected for my lecture this evening. We shall find the effects of the two opposing forces—that of heredity or conformation to ancestral characters, and that of adaptation to changed environment, whether brought about by the method of natural selection or otherwise—distinctly written in almost every part of their structure. Scarcely anywhere in the animal kingdom do we see so many cases of the persistence of rudimentary and apparently useless organs, those marvellous and suggestive phenomena which at one time seemed hopeless enigmas, causing despair to those who tried to unravel their meaning, looked upon as mere will-o'-the-wisps, but now eagerly welcomed as beacons of true light, casting illuminating beams upon the dark and otherwise impenetrable paths through which the organism has travelled on its way to reach the goal of its present condition of existence.

It is chiefly to these rudimentary organs of the Cetacea and to what we may learn from them that I propose to call your attention. In each case the question may well be asked, granted that they are, as they appear to be, useless, or nearly so, to their present possessors, insignificant, imperfect, in fact *rudimentary*, as compared with the corresponding or homologous parts of other animals, are they survivals, remnants of a past condition, become useless owing to change of circumstances and environment, and undergoing the process of gradual degeneration, preparatory to their final removal from an organism to which they are only, in however small a degree, an incumbrance, or are they incipient structures, beginnings of what may in future become functional and important parts of the economy? These questions will call for an attempt at least at solution in each case as we proceed.

Before entering upon details, it will be necessary to give some general idea of the position, limits, and principal modifications of the group of animals from which the special illustrations will be drawn. The term "whale" is commonly but vaguely applied to all the larger

and middle-sized Cetacea, and though such smaller species as the dolphins and porpoises are not usually spoken of as whales, they may for all intents and purposes of zoological science be included in the term, and will come within the range of the present subject. Taken all together the *Cetacea* constitute a perfectly distinct and natural order of mammals, characterised by their purely aquatic mode of life and external fishlike form. The body is fusiform, passing anteriorly into the head without any distinct constriction or neck, and posteriorly tapering off gradually towards the extremity of the tail, which is provided with a pair of lateral pointed expansions of skin supported by dense fibrous tissue, called "flukes," forming together a horizontally-placed, triangular propelling organ. The fore-limbs are reduced to the condition of flattened ovoid paddles, incased in a continuous integument, showing no external sign of division into arm, forearm, and hand, or of separate digits, and without any trace of nails. There are no vestiges of hind-limbs visible externally. The general surface of the body is smooth and glistening, and devoid of hair. In nearly all species a compressed median dorsal fin is present. The nostrils open separately or by a single crescentic valvular aperture, not at the extremity of the snout, but near the vertex.

Animals of the order *Cetacea* abound in all known seas, and some species are inhabitants of the larger rivers of South America and Asia. Their organisation necessitates their life being passed entirely in the water, as on the land they are absolutely helpless; but they have to rise very frequently to the surface for the purpose of respiration. They are all predaceous, subsisting on living animal food of some kind. One genus alone (*Orca*) eats other warm-blooded animals, as seals and even members of its own order, both large and small. Some feed on fish, others on small floating crustacea, pteropods, and medusæ, while the staple food of many is constituted of the various species of Cephalopods, chiefly *Loligo* and other *Teuthidæ*, which must abound in some seas in vast numbers, as they form almost the entire support of some of the largest members of the order. With some exceptions the Cetacea generally are timid, inoffensive animals, active in their movements, sociable and gregarious in their habits.

Among the existing members of the order there are two very distinct types—the Toothed Whales, or *Odontoceti*, and the Baleen Whales, or *Mystacoceti*, which present throughout their organisation most markedly distinct structural characters, and have in the existing state of nature no transitional forms. The extinct *Zeuglodon*, so far as its characters are known, does not fall into either of these groups as now constituted, but is in some respects intermediate, and in others more resembles the generalised mammalian type.

The important and interesting problem of the origin of the Cetacea and their relations to other forms of life is at present involved in the greatest obscurity. They present no more signs of affinity with any of the lower classes of vertebrated animals than do many of the members of their own class. Indeed, in all that essentially dis-

tinguishes a mammal from one of the oviparous vertebrates, whether in the osseous, nervous, vascular, or reproductive systems, they are as truly mammalian as any, even the highest, members of the class. Any supposed signs of inferiority are, as we shall see, simply modifications in adaptation to their peculiar mode of life. Similar modifications are met with in another quite distinct group of mammalia, the *Sirenia* (Dugongs and Manatees), and also, though in a less complete degree, in the aquatic Carnivora or seals. But these do not indicate any community of origin between these groups and the Cetacea. In fact, in the present state of our knowledge, the Cetacea are absolutely isolated, and little satisfactory reason has ever been given for deriving them from any one of the existing divisions of the class rather than from any other. The question has indeed often been mooted whether they have been derived from land mammals at all, or whether they may not be the survivors of a primitive aquatic form which was the ancestor not only of the whales, but of all the other members of the class. The materials for—I will not say solving—but for throwing some light upon this problem, must be sought for in two regions—in the structure of the existing members of the order, and in its past history, as revealed by the discovery of fossil remains. In the present state of science it is chiefly on the former that we have to rely, and this therefore will first occupy our attention.

One of the most obvious external characteristics by which the mammalia are distinguished from other classes of vertebrates is the more or less complete clothing of the surface by the peculiar modification of epidermic tissue called hair. The Cetacea alone appear to be exceptions to this generalisation. Their smooth, glistening exterior is, in the greater number of species, at all events in adult life, absolutely bare, though the want of a hairy covering is compensated for functionally by peculiar modifications of the structure of the skin itself, the epidermis being greatly thickened, and a remarkable layer of dense fat being closely incorporated with the tissue of the derm or true skin: modifications admirably adapted for retaining the warmth of the body, without any roughness of surface which might occasion friction and so interfere with perfect facility of gliding through the water. Close examination, however, shows that the mammalian character of hairiness is not entirely wanting in the Cetacea, although it is reduced to a most rudimentary and apparently functionless condition. Scattered, small, and generally delicate hairs have been detected in many species, both of the toothed and of the whalebone whales, but never in any situation but on the face, either in a row along the upper lip, around the blowholes or on the chin, apparently representing the large, stiff "vibrissæ" or "whiskers" found in corresponding situations in many land mammals. In some cases these seem to persist throughout the life of the animal; more often they are only found in the young or even the fœtal state. In some species they have not been detected at any age.

Eschricht and Reinhardt counted in a new-born Greenland Right Whale (*Balæna mysticetus*) sixty-six hairs near the extremity of the upper jaw, and about fifty on each side of the lower lip, as well as a few around the blowholes, where they have also been seen in *Megaptera longimana* and *Balænoptera rostrata.* In a large Rorqual (*Balænoptera musculus*), quite adult and sixty-seven feet in length, stranded in Pevensey Bay in 1865, there were twenty-five white, straight, stiff hairs about half an inch in length, scattered somewhat irregularly on each side of the vertical ridge in which the chin terminated, extending over a space of nine inches in height and two and a half inches in breadth. The existence of these rudimentary hairs must have some significance beyond any possible utility they may be to the animal. Perhaps some better explanation may ultimately be found for them, but it must be admitted that they are extremely suggestive that we have here a case of heredity or conformation to a type of ancestor with a full hairy clothing, just on the point of yielding to complete adaptation to the conditions in which whales now dwell.

In the organs of the senses the Cetacea exhibit some remarkable adaptive modifications of structures essentially formed on the Mammalian type, and not on that characteristic of the truly aquatic Vertebrates, the fishes, which, if function were the only factor in the production of structure, they might be supposed to resemble.

The modifications of the organs of sight do not so much affect the eyeball as the accessory apparatus. To an animal whose surface is always bathed with fluid, the complex arrangement which mammals generally possess for keeping the surface of the transparent cornea moist and protected, the movable lids, the nictitating membrane, the lachrymal gland, and the arrangements for collecting and removing the superfluous tears when they have served their function cannot be needed, and hence we find these parts in a most rudimentary condition or altogether absent. In the same way the organ of hearing in its essential structure is entirely mammalian, having not only the sacculi and semicircular canals common to all but the lowest vertebrates, but the cochlea, and tympanic cavity with its ossicles and membrane, all, however, buried deep in the solid substance of the head; while the parts specially belonging to terrestrial mammals, those which collect the vibrations of the sound travelling through air, the pinna and the tube which conveys it to the sentient structures within are entirely or practically wanting. Of the pinna or external ear there is no trace. The meatus auditorius is certainly there, reduced to a minute aperture in the skin like a hole made by the prick of a pin, and leading to a tube so fine and long that it cannot be a passage for either air or water, and therefore can have no appreciable function in connection with the organ of hearing, and must be classed with the other numerous rudimentary structures that whales exhibit.

The organ of smell, when it exists, offers still more remarkable evidence of the origin of the Cetacea. In fishes this organ is specially

adapted for the perception of odorous substances permeating the water; the terminations of the olfactory nerves are spread over the inner plicated surface of a cavity near the front part of the nose, to which the fluid in which the animals swim has free access, although it is quite unconnected with the respiratory passages. Mammals, on the other hand, smell substances with which the atmosphere they breathe is impregnated; their olfactory nerve is distributed over the more or less complex foldings of the lining of a cavity placed more deeply in the head, but in immediate relation to the passages through which air is continually driven to and fro on its way to the lungs in respiration, and therefore in a most favourable position for receiving impressions from substances floating in that air. The whalebone whales have an organ of smell exactly on the mammalian type, but in a rudimentary condition. The perception of odorous substances diffused in the air, upon which many land mammals depend so much for obtaining their food, or for protection from danger, can be of little importance to them. In the more completely modified Odontocetes the olfactory apparatus, as well as that part of the brain specially related to the function of smell, is entirely wanting, but in both groups there is not the slightest trace of the specially aquatic olfactory organ of fishes. Its complete absence and the vestiges of the aërial organ of land mammals found in the Mystacocetes are the clearest possible indications of the origin of the Cetacea from air-breathing and air-smelling terrestrial mammalia. With their adaptation to an aquatic mode of existence, organs fitted only for smelling in air became useless, and so have dwindled or completely disappeared. Time and circumstances have not permitted the acquisition of anything analogous to the specially aquatic smelling apparatus of fishes, the result being that whales are practically deprived of whatever advantage this sense may be to other animals.

It is characteristic of the greater number of mammalia to have their jaws furnished with teeth having a definite structure and mode of development. In all the most typical forms these teeth are limited in number, not exceeding eleven on each side of each jaw, or forty-four in all, and are differentiated in shape in different parts of the series, being more simple in front, broader and more complex behind. Such a dentition is described as "heterodont." In most cases also there are two distinct sets of teeth during the lifetime of the animal, constituting a condition technically called "diphyodont."

All the Cetacea present some traces of teeth, which in structure and mode of development resemble those of mammals, and not those of the lower vertebrated classes, but they are always found in a more or less imperfect state. In the first place, at all events in existing species, they are never truly heterodont, all the teeth of the series resembling each other more or less or belonging to the condition called "homodont," and not obeying the usual numerical rule, often falling short of, but in many cases greatly exceeding it. The most typical Odontocetes, or toothed whales, have a large number of similar,

simple, conical, recurved, pointed teeth, alike on both sides and in the upper and under jaws, admirably adapted for catching slippery, living prey, such as fish, which are swallowed whole without mastication. In one genus (*Pontoporia*) there may be as many as sixty of such teeth on each side of each jaw, making 240 in all. The more usual number is from twenty to thirty. These teeth are never changed, being "monophyodont," and they are, moreover, less firmly implanted in the jaws than in land mammals, having never more than one root, which is set in an alveolar socket which is generally wide and loosely fitting, though perfectly sufficient for the simple purpose which the teeth have to serve.

Most singular modifications of this condition of dentition are met with in different genera of toothed whales, chiefly the result of suppression, sometimes of suppression of the greater number, combined with excessive development of a single pair. In one large group, the Ziphioids, although minute rudimentary teeth are occasionally found in young individuals, and sometimes throughout life, in both jaws, in the adults the upper teeth are usually entirely absent, and those of the lower jaw reduced to two, which may be very large and projecting like tusks from the mouth, as in *Mesoplodon*, or minute and entirely concealed beneath the gums, as in *Hyperoodon*,—an animal which is for all practical purposes toothless, yet in which a pair of perfectly formed though buried teeth remain throughout life, wonderful examples of the persistence of rudimentary and to all appearance absolutely useless organs. Among the *Delphinidæ* similar cases are met with. In the genus *Grampus* the teeth are entirely absent in the upper, and few and early deciduous in the lower jaw. But the Narwhal exceeds all other Cetaceans, perhaps all other vertebrated animals, in the specialisation of its dentition. Besides some irregular rudimentary teeth found in the young state, the entire dentition is reduced to a single pair, which lie horizontally in the upper jaw, and both of which in the female remain permanently concealed within the bone, so that this sex is practically toothless, while in the male the right tooth usually remains similarly concealed and abortive, and the left is immensely developed, attaining a length equal to more than half that of the entire animal, projecting horizontally from the head in the form of a cylindrical or slightly tapering pointed tusk, with the surface marked by spiral grooves or ridges.

The meaning and utility of some of these strange modifications it is impossible, in the imperfect state of our knowledge of the habits of the Cetacea, to explain, but the fact that in almost every case a more full number of rudimentary teeth is present in early stages of existence, which either disappear, or remain as concealed and functionless organs, points to the present condition in the aberrant and specialised forms as being one derived from the more generalised type, in which the teeth were numerous and equal.

The Mystacocetes, or Whalebone Whales, are distinguished by entire absence of teeth, at all events after birth. But it is a remark-

able fact, first demonstrated by Geoffroy St. Hilaire, and since amply confirmed by Cuvier, Eschricht, Julin, and others, that in the fœtal state they have numerous minute calcified teeth lying in the dental groove of both upper and lower jaws. These attain their fullest development about the middle of fœtal life, after which period they are absorbed, no trace of them remaining at the time of birth. Their structure and mode of development has been shown to be exactly that characteristic of ordinary mammalian teeth, and it has also been observed that those at the posterior part of the series are larger, and have a bilobed form of crown, while those in front are simple and conical, a fact of considerable interest in connection with speculations as to the history of the group.

It is not until after the disappearance of these teeth that the baleen, or whalebone, makes its appearance. This remarkable structure, though, as will be presently shown, only a modification of a part existing in all mammals, is, in its specially developed condition as baleen, peculiar to one group of whales. It is therefore perfectly in accord with what might have been expected, that it is comparatively late in making its appearance. Characters that are common to a large number of species appear early, those that are special to a few, at a late period; alike both in the history of the race and of the individual.

Baleen consists of a series of flattened, horny plates, several hundred in number, on each side of the palate, separated by a bare interval along the middle line. They are placed transversely to the long axis of the palate, with very short spaces between them. Each plate or blade is somewhat triangular in form, with the base attached to the palate, and the apex hanging downwards. The outer edge of the blade is hard and smooth, but the inner edge and apex fray out into long, bristly fibres, so that the roof of the whale's mouth looks as if covered with hair, as described by Aristotle. The blades are longest near the middle of the series, and gradually diminish towards the front and back of the mouth. The horny plates grow from a dense fibrous and highly vascular matrix, which covers the palatal surface of the maxillæ, and which sends out lamellar processes, one of which penetrates the base of each blade. Moreover, the free edge of each of these processes is covered with very long vascular thread-like papillæ, one of which forms the central axis of each of the hair-like epidermic fibres of which the blade is mainly composed. A transverse section of fresh whalebone shows that it is made up of numbers of these soft vascular papillæ, circular in outline, each surrounded by concentrically arranged epidermic cells, the whole bound together by other epidermic cells, which constitute the smooth cortical (so-called "enamel") surface of the blade, and which, disintegrating at the free edge, allows the individual fibres to become loose and to assume the hair-like appearance spoken of before. These fibres differ from hairs in not being formed in depressed follicles in the enderon, but rather resemble those of which the horn of the rhinoceros is composed. The

blades are supported and bound together for a certain distance from their base, by a mass of less hardened epithelium, secreted by the surface of the palatal membrane or matrix of the whalebone in the intervals of the lamellar processes. This is the "intermediate substance" of Hunter, the "gum" of the whalers.

The function of the whalebone is to strain the water from the small marine mollusks, crustaceans, or fish upon which the whales subsist. In feeding they fill the immense mouth with water containing shoals of these small creatures, and then, on their closing the jaws and raising the tongue, so as to diminish the cavity of the mouth, the water streams out through the narrow intervals between the hairy fringe of the whalebone blades, and escapes through the lips, leaving the living prey to be swallowed. Almost all the other structures to which I am specially directing your attention are, as I have mentioned, in a more or less rudimentary state in the Cetacea; the baleen, on the other hand, is an example of an exactly contrary condition, but an equally instructive one, as illustrating the mode in which nature works in producing the infinite variety we see in animal structures. Although appearing at first sight an entirely distinct and special formation, it evidently consists of nothing more than the highly modified papillæ of the lining membrane of the mouth, with an excessive and cornified epithelial development.

The bony palate of all mammals is covered with a closely adhering layer of fibrovascular tissue, the surface of which is protected by a coating of non-vascular epithelium, the former exactly corresponding to the derm or true skin, and the latter to the epiderm of the external surface of the body. Sometimes this membrane is perfectly smooth, but it is more often raised into ridges, which run in a direction transverse to the axis of the head, and are curved with the concavity backwards; the ridges moreover do not extend across the middle line, being interrupted by a median depression or *raphé*. Indications of these ridges are clearly seen in the human palate, but they attain their greatest development in the Ungulata. In oxen, and especially in the giraffe, they form distinct laminæ, and their free edges develop a row of pointed papillæ, giving them a pectinated appearance. Their epithelium is thick, hard, and white, though not horny. Although the interval between the structure of the ridges in the giraffe's palate and the most rudimentary form of baleen at present known is great, there is no difficulty in seeing that the latter is essentially a modification of the former, just as the hoof of the horse, with its basis of highly developed vascular laminæ and papillæ, and the resultant complex arrangement of the epidermic cells, is a modification of the simple nail or claw of other mammals, or as the horn of the rhinoceros is only a modification of the ordinary derm and epiderm covering the animal's body differentiated by a local exuberance of growth.

Though the early stages by which whalebone has been modified from more simple palate structures are entirely lost to our sight, probably for ever, the conditions in which it now exists in different

species of whales, show very marked varieties of progress, from a simple comparatively rudimental and imperfect condition, to what is perhaps the most wonderful example of mechanical adaptation to purpose known in any organic structure. These variations are worth dwelling upon for a few minutes, as they illustrate in an excellent manner the gradual modifications that may take place in an organ, evidently in adaptation to particular requirements, the causation of which can be perfectly explained upon Darwin's principle of natural selection.

In the Rorquals or fin-whales (genus *Balænoptera*), found in almost all seas, and so well known off our own coasts, the largest blades in an animal 70 feet long do not exceed 2 feet in length, including their hairy terminations; they are in most species of a pale horn colour, and their structure is coarse and inelastic, separating into thick, stiff fibres, so that they are of no value for the ordinary purposes to which whalebone is applied to the arts. These animals feed on fish of considerable size, from herrings up to cod, and for foraging among shoals of these creatures the construction of their mouth and the structure of their baleen is evidently sufficient. This is the type of the earliest known extinct forms of whales, and it has continued to exist, with several slight modifications, to this day, because it has fulfilled one purpose in the economy of nature. Other purposes for which it was not sufficient have been supplied by gradual changes taking place, some of the stages of which are seen in the intermediate conditions still exhibited in the Megaptera, and in the Atlantic and Southern Right Whales. Before describing the extreme modifications in the direction of complexity, I may mention, to show the range at present presented in the development of baleen, that there has lately been discovered in the North Pacific a species called by the whalers the Californian Grey Whale (*Rachianectes glaucus*), which shows the opposite extreme of simplicity. The animal is from 30 to 40 feet in length; the baleen blades are only 182 on each side (according to Scammon) and far apart, very short (the longest being from 14 to 16 inches in length), light brown or nearly white in colour, and still more coarse in grain and inelastic than that of the Rorquals. The food of these whales is not yet known with certainty. They have been seen apparently seeking for it along soft bottoms of the sea, and fuci and mussels have been found in their stomachs.

In the Greenland Right Whale of the circumpolar seas, the Bowhead of the American whalers (*Balæna mysticetus*), all the peculiarities which distinguish the head and mouth of the whales from other mammals have attained their greatest development. The head is of enormous size, exceeding one-third of the whole length of the creature. The cavity of the mouth is actually larger than that of the body, thorax, and abdomen together. The upper jaw is very narrow, but greatly arched from before backwards, to increase the height of the cavity and allow for the great length of the baleen, the enormous rami of the mandibles are widely separated posteriorly, and have a

still further outward sweep before they meet at the symphysis in front, giving the floor of the mouth the shape of an immense spoon. The baleen blades attain the number of 350 or more on each side, and those in the middle of the series have a length of ten or even twelve feet. They are black in colour, fine and highly elastic in texture, and fray out at the inner edge and ends into long, delicate, soft, almost silky, but very tough hairs.

How these immensely long blades depending vertically from the palate were packed into a mouth the height of which was scarcely more than half their length, was a mystery not solved until a few years ago. Captain David Gray, of Peterhead, at my request, first gave us a clear idea of the arrangement of the baleen in the Greenland whale, and showed that the purpose of its wonderful elasticity was not primarily at least the benefit of the corset and umbrella makers, but that it was essential for the correct performance of its functions. It may here be mentioned that the modification of the mouth structure of the Right Whale is entirely in relation to the nature of its food. It is by this apparatus that it is enabled to avail itself of the minute but highly nutritious crustaceans and pteropods which swarm in immense shoals in the seas it frequents. The large mouth enables it to take in at one time a sufficient quantity of water filled with these small organisms, and the length and delicate structure of the baleen provides an efficient strainer or hair sieve by which the water can be drained off. If the baleen were, as in the Rorquals, short and rigid, and only of the length of the aperture between the upper and lower jaws when the mouth was shut, when the jaws were separated a space would be left beneath it through which the water and the minute particles of food would escape together. But instead of this, the long, slender, brush-like ends of the whalebone blades, when the mouth is closed, fold back, the front ones passing below the hinder ones in a channel lying between the tongue and the bone of the lower jaw. When the mouth is opened their elasticity causes them to straighten out like a bow that is unbent, so that at whatever distance the jaws are separated, the strainer remains in perfect action, filling the whole of the interval. The mechanical perfection of the arrangement is completed by the great development of the lower lip, which rises stiffly above the jaw-bone, and prevents the long, slender, flexible ends of the baleen being carried outwards by the rush of water from the mouth, when its cavity is being diminished by the closure of the jaws and raising of the tongue. The interest and admiration excited by the contemplation of such a beautifully adjusted piece of mechanism is certainly heightened by the knowledge that it has been brought about by the gradual adaptation and perfection of structures common to the whole class of animals to which the whale belongs.

Few points of the structure of whales offer so great a departure from the ordinary mammalian type as the limbs. The fore-limbs are reduced to the condition of simple paddles or oars, variously shaped, but always flattened and more or less oval in outline. They are

freely movable at the shoulder-joint, where the humerus or upper-arm bone articulates with the shoulder-blade in the usual manner, but beyond this point, except a slight flexibility and elasticity, there is no motion between the different segments. The bones are all there, corresponding in number and general relations with those of the human or any other mammalian arm, but they are flattened out, and their contiguous ends, instead of presenting hinge-like joints, come in contact by flat surfaces, united together by strong ligamentous bands, and all wrapped up in an undivided covering of skin, which allows externally of no sign of the separate and many-jointed fingers seen in the skeleton.

Up to the year 1865 it was generally thought that there was nothing to be found between this bony framework and the covering skin, with its inner layer of blubber, except dense fibrous tissue, with blood-vessels and nerves sufficient to maintain its vitality. Dissecting a large Rorqual, 67 feet in length, upon the beach of Pevensey Bay in that year, I was surprised to find lying upon the bones of the fore-arm well-developed muscles, the red fibres of which reached nearly to the lower end of these bones, ending in strong tendons, passing to, and radiating out on, the palmar surface of the hand. Circumstances then prevented me following out the details of their arrangement and distribution, but not long afterwards Professor Struthers, of Aberdeen, had an opportunity of carefully dissecting the fore-limb of another whale of the same species, and he has recorded and figured his observations in the 'Journal of Anatomy' for November 1871. He found on the internal or palmar aspect of the limb three distinct muscles corresponding in attachments to the flexor carpi ulnaris, the flexor profundus digitorum, and the flexor longus pollicis of man, and on the opposite side but one, the extensor communis digitorum.* Large as these muscles actually are, yet, compared with the size of the animal, they cannot but be regarded as rudimentary, and being attached to bones without regular joints and firmly held together by unyielding tissues, their functions must be reduced almost to nothing. But rudimentary as the muscles of the Fin-whales are, lower stages of degradation of the same structures are found in other members of the group. In some they are indeed present in form, but their muscular structure is gone, and they are reduced in most of the toothed whales to mere fibrous bands, scarcely distinguishable from the surrounding tissue which connects the inner surface of the skin with the bone. It is impossible to contemplate these structures without having the conviction forced home that here are the remains of parts once of use to their possessor, now, owing to the complete change of purpose and mode of action of the limb, reduced to a condition of atrophy verging on complete disappearance.

The changes that have taken place in the hind-limbs are even

* The muscles of the fore-arm of an allied species, *Balænoptera rostrata*, were described by Macalister in 1868, and Perrin in 1870.

more remarkable. In all known Cetacea (unless *Platanista* be really an exception) a pair of slender bones are found suspended a short distance below the vertebral column, but not attached to it, about the part where the body and the tail join. In museum skeletons these bones are often not seen, as, unless special care has been taken in the preparation, they are apt to get lost. They are, however, of much importance and interest, as their relations to surrounding parts show that they are the rudimentary representatives of the pelvic or hip bones, which in other mammals play such an important part in connecting the hind-limbs with the rest of the skeleton. The pelvic arch is thus almost universally present, but of the limb proper there is, as far as is yet known, not a vestige in any of the large group of toothed whales, not even in the great Cachalot or Sperm Whale, although it should be mentioned that it has never been looked for in that animal with any sort of care. With the Whalebone Whales, however, at least to some of the species, the case is different. In these animals there are found, attached to the outer and lower side of the pelvic bone, other elements, bony or only cartilaginous as the case may be, clearly representing rudiments of the first and in some cases the second segment of the limb, the thigh or femur, and the leg or tibia. In the small *Balænoptera rostrata* a few thin fragments of cartilage, imbedded in fibrous tissue attached to the side of the pelvic bone, constitute the most rudimentary possible condition of a hind-limb, and could not be recognised as such but for their analogy with other allied cases. In the large Rorqual, *Balænoptera musculus*, 67 feet long, previously spoken of, I was fortunate enough in 1865 to find attached by fibrous tissue to the side of the pelvic bone (which was sixteen inches in length) a distinct femur, consisting of a nodule of cartilage of a slightly compressed, irregularly oval form, and not quite one inch and a half in length. Other specimens of the same animal dissected by Van Beneden and Professor Struthers have shown the same; in one case, partial ossification had taken place. In the genus *Megaptera* a similar femur has been described by Eschricht; and the observations of Reinhardt have shown that the Greenland Right Whale (*Balæna mysticetus*) has not only a representative of the femur developed far more completely than in the Rorqual, being from six to eight inches in length and completely ossified, but also a second smaller and more irregularly formed bone, representing the tibia. Our knowledge of these parts in this species has recently been greatly extended by the researches of Dr. Struthers, who has published in the 'Journal of Anatomy' for 1881 a most careful and detailed account of the dissection of several specimens, showing the amount of variation to which these bones (as with most rudimentary structures) are liable in different individuals, and describing for the first time their distinct articulation one with the other by synovial joints and capsular ligaments, and also the most remarkable and unlooked-for presence of muscles passing from one bone to the other, representing the adductors and flexors of mammals with completely

developed limbs, but so situated that it is almost impossible to conceive that they can be of any use; the whole limb, such as it is, being buried deep below the surface, where any movement, except of the most limited kind, must be impossible. Indeed, that the movement is very limited and of no particular importance to the animal was shown by the fact that in two out of eleven whales dissected the hip-joint was firmly anchylosed (or fixed by bony union) though without any trace of disease. In the words of Dr. Struthers, "Nothing can be imagined more useless to the animal than rudiments of hind-legs entirely buried beneath the skin of a whale, so that one is inclined to suspect that these structures must admit of some other interpretation. Yet, approaching the inquiry with the most sceptical determination, one cannot help being convinced, as the dissection goes on, that these rudiments really are femur and tibia. The functional point of view fails to account for their presence. Altogether they present for contemplation a most interesting instance of those significant parts, rudimentary structures."

We have here a case in which it is not difficult to answer the question before alluded to, often asked with regard to rudimentary parts: Are they disappearing or are they incipient organs? We can have no hesitation in saying that they are the former. All we know of the origin of limbs shows that they commence as outgrowths upon the surface of the body, and that the first-formed portions are the most distal segments. The limb, as proved by its permanent state in the lowest Vertebrates, and by its embryological condition in higher forms, is at first a mere projection or outward fold of the skin, which, in the course of development, as it becomes of use in moving or supporting the animal, acquires the internal framework which strengthens it and perfects its functions. It would be impossible on any theory of causation yet known, to conceive of a limb gradually developed from within outwards. On the other hand, its disappearance would naturally take place in the opposite direction; projecting parts which had become useless, being in the way, would, like all the other prominences on the surface of the whales, hair, ears, &c., be removed, while the most internal, offering far less interference with successful carrying on the purposes of life, would be the last to disappear, lingering, as in the case of the Greenland Whale, long enough to reveal their wonderful history to the anatomist who has been fortunate enough to possess the skill and the insight to interpret it.

Time will not allow of more illustrations drawn from the structure of existing Cetacea; we turn next to what the researches of palaeontology teach of the past history of the order. Unfortunately this does not at present amount to very much. As is the case with nearly all other orders of mammals, we know nothing of their condition, if they existed, in the mesozoic age. Even in the Cretaceous seas, the deposits at the bottom of which are so well adapted to preserve the remains of the creatures which swam in them, not a fragment of any whale or whale-like animal has been found. The earliest Cetaceans

of whose organisation we have any good evidence. are the Zeuglodons of the Eocene formations of North America. These were creatures whose structure, as far as we know it, was intermediate between that of the existing suborders of whales, having the elongated nasal bones and anterior position of the nostrils of the Mystacocetes, with the teeth of the Odontocetes, and with some characters more like those of the generalised mammalian type, than of any of the existing forms. In fact *Zeuglodon* is precisely what we might have expected *a priori* an ancestral form of whale to have been. The remarkable smallness of its cerebral cavity, compared with the jaws and the rest of the skull, so different from that of modern Cetaceans, is exactly paralleled in the primitive types of other groups of mammals. The teeth are markedly differentiated in different parts of the series. In the anterior part of both jaws they are simple, conical, or slightly compressed and sharp pointed. The first three of the upper jaw are distinctly implanted in the premaxillary bone, and so may be reckoned as incisors. The tooth which succeeds, or the canine, is also simple and conical, but it does not greatly exceed the others in size. This is followed by five teeth with two distinct roots and compressed pointed crowns, with denticulated cutting edges. It has been thought that there was evidence of a vertical succession of the molar teeth, as in diphyodont mammals, but the proof of this is not quite satisfactory. Unfortunately the structure of the limbs is most imperfectly known. A mutilated humerus has given rise to many conjectures; to some anatomists it appears to indicate freedom of motion at the elbow-joint, while to others its characters seem to be those of the ordinary Cetacea. Of the structure of the pelvis and hind-limb we are at present in ignorance.

From the middle Miocene period fossil Cetacea are abundant, and distinctly divided into the two groups now existing. The Mystacocetes, or Whalebone Whales, of the Miocene seas were, as far as we know now, only *Balænopteræ*, some of which (as the genus *Cetotherium*) were, in the elongated flattened form of the nasal bones, the greater distance between the occipital and frontal bones at the top of the head, and the greater length of the cervical vertebræ, more generalised than any now existing. In the shape of the mandible also, Van Beneden, to whose researches we are chiefly indebted for a knowledge of these forms, discerns some approximation to the Odontocetes. Right Whales (*Balæna*) have not been found earlier than the Pliocene period, and it is interesting to note that instead of the individuals diminishing in bulk as we approach the times we live in, as with many other groups of animals, the contrary has been the case, no known extinct species of whales equalling in size those that are now to be met with in the ocean. The size of whales, as of all other things whose most striking attribute is magnitude, has been greatly exaggerated; but when reduced to the limits of sober fact, the Greenland Right Whale of 50 feet long, the Sperm Whale of 60, and the Great Northern Rorqual (*Balænoptera Sibbaldii*) of 80, exceed all

other organic structures known, past or present. Instead of living in an age of degeneracy of physical growth, we are in an age of giants, but it may be at the end of that age. For countless centuries impulses from within and the forces of circumstances from without have been gradually shaping the whales into their present wonderful form and gigantic size, but the very perfection of their structure and their magnitude combined, the rich supply of oil protecting their internal parts from cold, the beautiful apparatus of whalebone by which their nutrition is provided for, have been fatal gifts, which, under the sudden revolution produced on the surface of the globe by the development of the wants and arts of civilised man, cannot but lead in a few years to their extinction.

It does not need much foresight to divine the future history of whales, but let us return to the question with which we started, What was their probable origin?

In the first place, the evidence is absolutely conclusive that they were not originally aquatic in habit, but are derived from terrestrial mammals of fairly high organisation, belonging to the placental division of the class,—animals in which a hairy covering was developed, and with sense organs, especially that of smell, adapted for living on land; animals, moreover, with four completely-developed pairs of limbs on the type of the higher vertebrata, and not of that of fishes. Although their teeth are now of the simple homodont and diphyodont type, there is much evidence to show that this has taken place by the process of degradation from a more perfect type, even the fœtal teeth of whalebone whales showing signs of differentiation into molars and incisors, and many extinct forms, not only the Zeuglodons, but also true dolphins, as the Squalodons, having a distinct heterodont dentition, the loss of which, though technically called a "degradation," has been a change in conformity to the habits and needs of the individuals. So much may be considered very nearly, if not quite, within the range of demonstrated facts, but it is in determining the particular group of mammals from which the Cetacea arose that greater difficulties are met with.

One of the methods by which a land mammal may have been changed into an aquatic one is clearly shown in the stages which still survive among the Carnivora. The seals are obviously modifications of the land Carnivora, the Otariæ, or sea-lions and sea-bears, being curiously intermediate. Many naturalists have been tempted to think that the whales represent a still further stage of the same kind of modification. So firmly has this idea taken root that in most popular works on zoology in which an attempt has been made to trace the pedigree of existing mammals, the Cetacea are definitely placed as offshoots of the Pinnipedia, which in their turn are derived from the Carnivora. But there is to my mind a fatal objection to this view. The seal of course has much in common with the whale, inasmuch as it is a mammal adapted for an aquatic life, but it has been converted to its general fish-like form by the peculiar development of its

hind-limbs into instruments of propulsion through the water; for though the thighs and legs are small, the feet are large and are the special organs of locomotion, the tail being quite rudimentary. The two feet applied together form an organ very like the tail of a fish or whale, and functionally representing it, but only functionally, for the time has I trust quite gone by when the Cetacea were defined as animals with the "hinder limbs united, forming a forked horizontal tail." In the whales, as we have seen, the hind-limbs are aborted and the tail developed into a powerful swimming organ. Now it is very difficult to suppose that when the hind-limbs had once become so well adapted to a function so essential to the welfare of the animal as that of swimming, they could ever have become reduced and their action transferred to the tail;—the animal must have been in a too helpless condition to maintain its existence during the transference, if it took place, as we must believe, gradually. It is far more reasonable to suppose that whales were derived from animals with large tails, which were used in swimming, eventually with such effect that the hind-limbs became no longer necessary, and so gradually disappeared. The powerful tail, with lateral cutaneous flanges, of an American species of otter (*Pteronura sandbachii*) or the still more familiar tail of the beaver, may give some idea of this member in the primitive *Cetacea*. I think that this consideration disposes of the principal argument in favour of the whales being related to the seals, as most of the other resemblances, such as those in the characters of their teeth, are evidently resemblances of analogy related to similarity of habit.

As pointed out long ago by Hunter, there are numerous points in the structure of the visceral organ of the Cetacea far more resembling those of the Ungulata than the Carnivora. These are the complex stomach, simple liver, respiratory organs, and especially the reproductive organs and structures relating to the development of the young. Even the skull of Zeuglodon, which has been cited as presenting a great resemblance to that of a seal, has quite as much likeness to one of the primitive pig-like Ungulates, except in the purely adaptive character of the form of the teeth.

Though there is, perhaps, generally more error than truth in popular ideas on natural history, I cannot help thinking that some insight has been shown in the common names attached to one of the most familiar of Cetaceans by those whose opportunities of knowing its nature have been greatest—"Sea-Hog," "Sea-Pig," or "Herring-Hog" of our fishermen, *Meerschwein* of the Germans, corrupted into the French "Marsouin," and also "Porcpoisson," shortened into "Porpoise."

The difficulty that might be suggested in the derivation of the Cetacea from the Ungulata, arising from the latter being at the present day mainly vegetable feeders, is not great, as the primitive Ungulates were probably omnivorous, as their least modified descendants, the pigs, are still; and the aquatic branch might easily have

gradually become more and more piscivorous, as we know from the structure of their bones and teeth, the purely terrestrial members have become by degrees more exclusively graminivorous.

One other consideration may remove some of the difficulties that may arise in contemplating the transition of land mammals into whales. The Gangetic Dolphin (*Platanista*) and the somewhat related *Inia* of South America, which retain several rather generalised mammalian characters, and are related to some of the earliest known European Miocene dolphins, are both to the present day exclusively fluviatile, being found in the rivers they inhabit almost up to their very sources, more than a thousand miles from the sea. May this not point to the freshwater origin of the whole group, and thus account for their otherwise inexplicable absence from the Cretaceous seas?

We may conclude by picturing to ourselves some primitive generalised, marsh-haunting animals with scanty covering of hair like the modern hippopotamus, but with broad, swimming tails and short limbs, omnivorous in their mode of feeding, probably combining water-plants with mussels, worms, and freshwater crustaceans, gradually becoming more and more adapted to fill the void place ready for them on the aquatic side of the borderland on which they dwelt, and so by degrees being modified into dolphin-like creatures inhabiting lakes and rivers, and ultimately finding their way into the ocean. Here the disappearance of the huge Enaliosaurians, the *Ichthyosauri* and *Plesiosauri*, which formerly played the part the Cetacea do now, had left them ample scope. Favoured by various conditions of temperature and climate, wealth of food supply, almost complete immunity from deadly enemies, and illimitable expanses in which to roam, they have undergone the various modifications to which the Cetacean type has now arrived, and gradually attained that colossal magnitude which we have seen was not always an attribute of the animals of this group.

Please to recollect, however, that this is a mere speculation, which may or may not be confirmed by subsequent palæontological discovery. Such speculations are, I trust, not without their use and interest, especially when it is distinctly understood that they are offered only as speculations and not as demonstrated facts.

[W. H. F.]

WEEKLY EVENING MEETING,

Friday, June 1, 1883.

Sir Frederick Pollock, Bart. M.A. Vice-President, in the Chair.

Frederick Pollock, Esq. M.A. LL.D.

The Forms and History of the Sword.

There seems to be a culminating point not only in all human arts, but in the fashion of particular instruments. And it so happens that the pre-eminent and typical instruments of war and of music attained their perfection at nearly the same time, in the first quarter of the eighteenth century. Within that period the violin, chief minister of the most captivating of the arts of peace, and the sword, the chosen weapon of skilled single combat and the symbol of military honour, assumed their final and absolute forms—forms on which no improvement has been found possible. Strangely enough, the parallel holds a step further. In each case, although nothing more could be added to the model or the workmanship, it was yet to be long before the full capacities of the instrument were developed. A quartet of Beethoven hardly differs more from the formal suites and gavottes of such composers as Rameau, than does the sword-play of the school of Prévost or Cordelois from the nicely balanced movements and counter-movements taught and figured in the works of De Liancour or Girard. Nor has fencing been without its modern romantic school; we may even say that it has had its Berlioz in the brilliant and eccentric De Bazancourt, a charming writer on the art, and—as he has been described to me by competent authority—*un tireur des plus fantaisistes.* And in both cases we may truly say that the period of academic formality was the indispensable predecessor of the more free and adventurous development of our own time. But before the modern small-sword could even exist—*the* sword, as it is called eminently and without addition in its land of adoption, *épée* as opposed to *sabre* —a long course of growth, variation, and experiment had to be run through. To give some general notion of the forms and history of the sword is what I shall now attempt; I say some notion, for the subject, narrow as it may seem at first sight, is one that marvellously grows upon consideration; and I can well understand that (if report says true) Captain Burton has found three volumes none too many for the compass of the exhaustive work which, after long preparation, he is about to give us. And though there are perhaps not many of us nowadays who would, like Claudio before he fell in love, walk ten mile a-foot to see a good armour, I think we shall find the story not without interest.

The sword is essentially a metal weapon. Here at the outset we are on disputable ground; one cannot take a part either way without differing from good authorities. But some part must be taken, and on this point I hold with General Pitt-Rivers. The larger wooden or stone weapons, clubs and the like, were not and could not be imitated in bronze in the early days of metal-work, for the one sufficient reason that metal was too scarce. We start then with spear-heads of hammered bronze, imitating the pointed flints which doubtless were still used for arrow-heads until bronze was cheap enough to be thrown or shot away without thought of recovering it. The general form of these spear-heads was a kind of pointed oval, a type which has continued with only minor variations in the greater part of the spears, pikes, and lances of historical times. It is difficult to say whether the spears thus headed were oftener used as missile or thrusting weapons, though the javelin has also forms peculiar to itself, of which the most famous example is the Roman *pilum*. In the semi-historical warfare of the Homeric poems the spear is almost always thrown; in the later historical period it is held fast as a pike; the Romans, carefully practical in all matters of military equipment, had different spears for different kinds of service. In mediæval Europe the missile use of spears had, I believe, disappeared altogether, except in the defence of walls and in naval combats. However these things may be, the need of a handier weapon than the spear for close quarters, and a readier and more certain one than the club, must have been felt at an early time. A spear broken off short would at once give a hand-weapon like the Zulu "stabbing assegai." When metal becomes more abundant, and skill in working it more common, such weapons are separately designed and made; the spear-head is enlarged into a blade, with but little alteration of form, and we have a bronze* dagger of the type known to English archæologists as "leaf-shaped," the characteristic type of the bronze period everywhere. Some of the Greek bronze daggers, indeed, are rather smaller than the full-sized spear-heads. With increasing command of metal the length of blade is increased; and we have in course of time a true sword. It is impossible to define where the dagger ends and the sword begins, but perhaps the metal-bladed weapon may fairly be called a sword when it is two feet long or upwards, and has a metal grip, or nucleus of a grip (the "tang" of the modern armourer), wrought in the same piece with it, and finished off with a counter-guard or pommel. It may be observed that the prehistoric armourers, as far as one can guess, had no theories as to the most effective length of their weapons. I believe the dimensions were determined (within the limits of practical handling by a man of average stature) almost wholly by the costliness of the material. In the later bronze and earlier iron

* It is not universally true that bronze was known and worked before other metals. Iron came first where, as in Africa, it was most accessible. But I speak here with a view to the European development only.

periods we find the blades attaining almost or quite the length of a modern sabre. And in like manner the bulging curvature towards the point appears not to have been adopted in order to give cutting power (which to some extent it does), but to be a mere imitation of the spear-head, which in turn owes its form to imitation of the earlier chipped flint points. However produced, and for whatever reasons retained, this leaf-shape is the continuing type of the Greek sword throughout ancient Greek history; and it is not only thus persistent, but now and then recurs at much later times in unexpected ways. It is exactly reproduced in a pattern of short sword for the French dismounted artilleryman, dated 1816, which may be seen in the Musée d'Artillerie at the Invalides, and in some recent experimental sword-bayonets.* As the blade lengthened, the leaf-shape was less marked, and in the days of the Roman empire, and the barbarian dynasties which were built up on its ruins, the symmetrical curvature had disappeared, leaving a straight and broad blade which became the European sword of the middle ages. Meanwhile the leaf-shape had thrown out other offshoots elsewhere. From the mediæval type of sword, or in some cases from one of these other forms, are derived all the weapons of this class now employed by the European races of man.

Even in the prehistoric period the leaf-shape underwent variations. There have lately been found at Mycenæ several sword (or rather dagger) blades of unknown antiquity, differing from the common pattern in being straight-edged; as likewise, it is worth while to note, are the swords figured on Assyrian sculptures, narrow and slender weapons mounted not unlike the Roman army sword, and apparently tapering to a point. These Mycenæan examples are elaborately decorated, and of the utmost interest as specimens of early artistic metal work. Two of them are considerably shorter than the others, and these are the most finely wrought.† The blades are covered with hunting scenes and figures of animals, partly real and partly fabulous; the style of the work is archaic, and both the general style and certain details suggest an Egyptian origin for the school from which it came, if not for the artists themselves. The figures are not wrought in one piece with the blades, but made separately and let in. Some process of the nature of enamelling is used in parts, and gold, or alloys of gold and silver of different shades, are employed to give

* The Londoner need not even trouble himself to walk into a museum, for the leaf-shaped Greek sword of classical times has been carefully copied from the best authorities in the weapon held by the statue at Hyde Park Corner taken from the group of the Dioscuri on Monte Cavallo, disfigured by a total perversion of the original motive, and absurdly re-named Achilles.

† Kumanudes, Ἀθήναιον, vol. ix. p. 162, and x. p. 309; Köhler, 'Mittheilungen des deutschen archäologischen Instituts in Athen,' vol. vii. p. 241. I am indebted to Professor Colvin for the communication of these papers and their illustrations, as also the monograph on the Roman soldier's equipment cited below.

variety, and indicate to some extent the natural colouring of the objects. Whether imported or produced by a naturalised school of craftsmen, arms so richly adorned cannot have been at any time otherwise than a luxury confined to chiefs of the highest rank. These are of an antiquity far greater than that of the Homeric poems; and in Homer there is nothing that would lead us to expect such work, though decorated scabbards and mountings are mentioned. Swords occur now and then as presents, but there is no trace of their being peculiarly valuable possessions, and still less of any peculiar feeling of honour being associated with them. The spear is the favourite weapon of Homer's mighty men, as witness the spear of Achilles which none but himself can cast. The sword is used only when the spear has failed, and seems to do little execution then. In historical Greece, and to some extent among the Romans, the military point of honour was bound up with the shield, probably because the abandonment of it was naturally the first action of defeated troops anxious to lighten themselves in retreat.

So far as anything can be inferred from the allusions of the Greek tragedians, and from a few historical details like the improvements in equipment introduced by Iphicrates, the sword had a better relative position among the arms of Greek warriors in post-Homeric times. Probably this was due to the supplanting of bronze by iron—a process which was complete so long before Thucydides wrote, that iron was in his language the natural and obvious material of weapons. To wear arms is for him to wear iron: in old times, he says, every man in Greece "wore iron" in every-day life, like the barbarians nowadays. But it is in the Roman armies that we find the first distinct evidence of the use of the sword being studied with anything like system. We learn from Vegetius—a writer of the late fourth century A.D., and of no great authority for his own sake, but likely enough to have preserved genuine traditions of the service—that the Roman soldier was assiduously practised in sword exercise. What is more important, the Romans had discovered the advantage of using the point, and regarded enemies who could only strike with the edge as contemptible. Vegetius assigns as reasons for this both the greater effectiveness of a thrust and the less exposure of the body and arm in delivering it; reasons which though not conclusive are plausible, and show that the matter had been thought out. Further, the Roman practice, notwithstanding the temptation to keep the shielded side foremost, was to advance the right side in attacking, as modern swordsmen do. The weapon was a thoroughly practical one: the straight and short blade was mounted in a hilt not unlike that of a Scottish dirk, scored with well-marked grooves for the fingers, and balanced with a substantial pommel: this last point, by the way, is too much neglected in our present military swords. A shorter and broader pattern was worn by superior officers, sometimes in a highly ornamented scabbard, of which there is a very fine specimen in the British Museum. Longer swords were used by the cavalry and by the foreign

troops in the Roman service.* There is no evidence, however, that the Romans ever attained the point of cultivating swordsmanship in the proper sense, that is, making the sword a defensive as well as an offensive arm.

After the fall of the Roman empire the sword in general use is a longer and larger weapon, but handled, we may suspect, with less skill and effect. It is straight, heavy, double-edged, and of varying length apparently determined by no rule beyond the strength or the fancy of the owner. A good historical specimen of this type is the sword of Charles the Great, exhibited in the Louvre. As often as not the earlier mediæval swords are rounded off at the end; and from this, as well as from the fact that some centuries later the "foining fence" of the Italian school was regarded as a wholly new thing, it appears that the Roman tradition of preferring the point to the edge had been lost or disregarded. There is every reason, indeed, to believe that the mediæval form is the continuance of a prehistoric one. Swords dug up in various parts of Europe from several feet of gravel show no essential difference of pattern from those which were common down to the sixteenth century. The hilts of the prehistoric swords do indeed affect (though not invariably) a shortness in the grip which seems to modern Europeans absurd, though a parallel to it may be found in modern Asiatic swords; and very short handles occur in European weapons as late as the thirteenth century. From three to three and a half inches, or sometimes even less, is all the room given to the hand. The modern European swordsman's grip is flexible; he requires free space and play for the fingers, and for the directing action of the thumb which is all but indispensable in using the point. The short grip is intended to give a tight-fitting and rigid grasp, so that the whole motion of the cut comes from the arm and shoulder; and this is the manner in which Oriental swords are still handled. Apart from this difference in the size of the grip, a mediæval knight's sword, or one of the Scottish swords to which the name of claymore (commonly usurped by the much later basket-hilted pattern) properly belongs, has little to distinguish it from the arms of unknown date which, for want of a more certain attribution, are vaguely called British in our museums. But one thing of great curiosity happened to the sword in the middle ages; it became a symbol of honour, an object almost of worship, the chosen seat and image of the sentiment of chivalry. This may be accounted for in part by the accident of the cross-guard seeming to the newly converted barbarians to invest it with a sacred character; I say accident, for the cross-guard is certainly prehistoric and therefore pre-Christian. Still the religious associations of the cross must have given a quite

---

* Lindenschmit, 'Tracht und Bewaffnung des römischen Heeres während der Kaiserzeit.' Braunschweig, 1882. Complete reconstructions of both Greek and Roman equipments of various periods (among others) may be seen in the excellent historical collection of *Costumes de guerre* in the Musée d'Artillerie of Paris.

new significance and importance to such customs as that of swearing by the sword—itself a widely spread one, and of extreme antiquity.* I think that other though not dissimilar influences also came into play. In the Old Testament the sword is much oftener mentioned than the spear, and is a recognised symbol of war and warlike power. Thus, to take one of the best known passages, we read in the forty-fifth Psalm, "Gird thee with thy sword upon thy thigh, O thou most mighty:" in the Vulgate, *Accingere gladio tuo super femur tuum, potentissime.* Now it is no matter of conjecture that such a passage deeply affected the mediæval imagination. These words are quoted by a man of peace, our own Bracton, writing in the thirteenth century, when he speaks of the king's power, and of the counsellors and barons who are his companions, girt with swords, assisting him to do judgment and justice. It seems hardly too fanciful to think that the fascination and pre-eminence of the sword which were at their height in Bracton's time, and are not extinct yet, were in some measure derived from that one triumphant note of the Psalmist. Not that others were wanting; there is the two-edged sword in the hands of the saints: *Exaltationes Dei in gutture eorum, et gladii ancipites in manibus eorum*, a verse that was in time to serve the Puritans as it had served the Crusaders.

But to follow out the associations of the sword with knighthood, semi-religious military vows and enterprises, and military honour in general, would be matter for a discourse of itself. Let us return to the fashion and development of the weapon. There was little variation from the eleventh to the sixteenth century, save that the decoration of the scabbard and mountings (of which I do not propose to speak) grew more elaborate with the growth of art and luxury, and that the average length tended to increase. After the twelfth century the sword is generally pointed as well as two-edged, and the point was sometimes used with effect. In a fourteenth century MS. in the British Museum, engraved in Hewitt's 'Ancient Armour and Weapons,' a mounted knight is delivering a thrust in quarte (as we now say), which completely pierces his adversary's shield. In the

* It is common among the Rájpúts, and is met with, in conjunction with peculiar formalities, among certain hill tribes. Wilbraham Egerton, 'Handbook of Indian Arms' (published by the India Office, 1880), pp. 77, 105–6. It is also a very old Teutonic custom. Grimm, 'Deutsche Rechtsalterthümer,' pp. 165, 896, cf. Ducange, *s.v. Juramentum* (*super arma*). The implied imprecation was probably, "May the god of war abandon me in fight if I swear falsely," hardly "May I perish by the sword," for it was held disgraceful to a free man to die otherwise than in battle. In the sixteenth century Spanish fencing-masters, on their admission to the guild, took an oath "super signum sanctæ crucis factum de pluribus ensibus," 'Revue archéologique,' vi. 589. Not unfrequently the sword itself was the object of worship; the feeling is more easily revived in fighting times, even now, than men of peace are apt to think, as Körner's well-known sword-song shows. Compare General Pitt-Rivers's Catalogue of his collection (Stationery Office, 1877), p. 102. Some of the formulas in Ducange suggest the meaning, "What I assert or promise I am ready to make good with the sword;" but this I suspect is a later rationalising of the original ceremony.

sixteenth century the blade is made narrower and lighter, and the sword-hand is for the first time adequately guarded. First, the plain cross-bar puts on various curved forms intended to arrest or entangle an enemy's blade with greater effect. Then rings project on either side of the root of the blade, and are worked, as time goes on, into a more or less complex system of convolutions according to the costliness of the weapon and the skill and fancy of the maker. These curved guards are known as *pas d'âne*, while the cross-pieces in the plane of the blade, now slender and elongated, and often curving towards the point, are called *quillons*. Next the guard throws up one or more branches, covering or encircling the exposed outer part of the hand. These branches form a shell or basket pattern, their ends are solidly joined to the pommel (after an interval of hesitating osculation, well exemplified in a sword now in the museum of the United Service Institution, which was borne by Cromwell at Drogheda), and nothing but a process of selection and simplification is now needed to produce all the modern patterns of sword-hilts. It was at Venice that the basket-hilt came first into regular use in the swords named *Schiavone*, from being worn by the Doge's body-guard (*Schiavoni*, Slavs, i. e. Dalmatians). In these it is of a flattened elliptical shape. The Scots, renowned before the middle of the sixteenth century for their careful choice of weapons, took up the model, and in the course of another generation or two developed it into the well-known basket-guard still used by our Highland regiments, the most complete protection for the swordsman's hand ever devised without undue loss of freedom. Meanwhile the *pas d'âne* solidifies into a hollowed disc or even a deep bell-shaped cup, the characteristic feature of the guard of the Spanish rapier and the modern duelling sword. One cannot help speaking of the works of men's hands, when one traces them in historical order through their several forms, as if they were organic and grew like flowers, or like variations of a natural species; and in truth it is not an idle conceit, for the development of design and workmanship answers to a real organic development in the men from whose brain and hand the work proceeds; every generation takes up from its fathers, if it is worthy of them, a new starting-point of imagination and aptitude, and the strange conservatism of the imitative faculty is a sure warrant of continuity.

The latter half of the sixteenth century was the time when the sword stood highest in artistic honour. Then it was that Holbein designed its ornaments for Henry VIII., and that Albert Dürer engraved a crucifixion on a plate of gold for the boss of a sword or dagger of the Emperor Maximilian's. Both the sword and its ornament disappeared at an early time, the prey of some greatly daring collector, and nothing is now known of their fate: the design survives, for impressions were taken as from an ordinary engraver's plate, and some are still in existence, though a good example is extremely rare. But in the true armourer's or swordsman's eyes the work even of a Holbein and a Dürer is only extraneous adornment,

and must yield in interest to the qualities of the blade. And at this time the sword-smith became again, as he had been in the ruder ages when metal working was the secret of a few craftsmen, a man of renown. In Spain, in France, in Germany, and in Italy, there rose up masters and schools of sword-cutlery. There was a time when the blades of Bordeaux and Poitiers had the best price in the English market; but soon those of Toledo, combining beauty, strength, and elasticity, gained that eminence of which the tradition still clings to them. Othello's "sword of Spain, the ice-brook's temper," was such an one as these now before us. And Shakespeare, be it noted, knew here, as always, exactly what he was speaking of; for it was long believed that the quality of the finest blades depended on their being tempered in mountain streams. Germany was not far behind in the race either; the Solingen blades, stouter and rougher than the Spanish ones, but for that reason fitter for common military service, made their trade-mark of a running wolf known throughout the north of Europe. The wolf, or hieroglyphic symbol that passed for one, was easily taken for a fox. Hence, it should seem, the cant name of fox for a sword, which is current in our Elizabethan literature. "O, Signieur Dew, thou diest on point of fox," cries Pistol to his captive on the field of Agincourt. A still greater reputation was gained by the strong and keen broadswords bearing the name of Andrea Ferara, long a puzzle to antiquaries from the want of positive knowledge whether he was of Italian or Spanish origin. The story that he was invited to Scotland by James V. appears to be mere guess-work. There exists, however, contemporary evidence that some time after 1580 two brothers, Giovan Donato and Andrea dei Ferari, were well-known sword-makers, working at Belluno in Friuli, the Illyrian territory of Venice; and this goes far to settle the question between Spain and Italy.* Probably the name of Ferara became a kind of trade-mark, and was used afterwards by many successors or imitators.

During this time the Spanish and Italian rapier was undergoing its peculiar development, and leading the way to the modern art of fencing. But this takes us out of the general line of history into a distinct branch. We have henceforth to consider the sword, not as the simple following out of a given primitive form, but as a weapon diverging from that form in two directions. It may be specialised as a cutting or as a thrusting arm. In the military sabre of our own time we find both qualities reconciled by a sufficiently effective compromise, but only after a long course of experiments.

For many centuries the armourers and swordsmen of the East have cultivated the edge at the expense of the point, and have attained a partly just and partly fabulous renown. The point, after being neglected since the days of the Romans, has made up its lost time in the West, and made it up triumphantly; for it is now admitted that the swordsman who would be a complete master of the edge must have

* 'Cornhill Magazine,' vol. xii. p. 192 (August 1865).

learnt the ways of the point also. Let us take the earlier stage first, as shown in the cutting swords of the East. Broadly speaking, their characteristic feature is a decidedly curved blade as opposed to the straight or nearly straight European form. Not that all old European swords are straight, or all Eastern swords curved. There are curved blades of mediæval and even earlier times (one prehistoric example is in the Copenhagen Museum), and one remarkable type of Indian sword, the Mahratta gauntlet sword (Paṭá), is quite straight; but the contrast holds good in the main. The object of curvature is to gain cutting power. When a straight sword strikes its object full, the direction of the stroke is at right angles to the length of the blade; and the amount of resistance, for a given velocity of stroke and substance to be cut, is measured by the acuteness of the angle shown by a transverse section of the cutting blade. The finer this angle, the less the resistance. In the case of an instrument not intended to bear rough usage or cut hard bodies, or much of any substance at one stroke (as a razor), it is only a question of workmanship how fine the angle can be made. But with a sword it is otherwise. Without a certain amount of thickness, the best steel blade would be too fragile or too flexible, so that in practice the limit up to which its cutting power can be increased by fining down the edge is soon reached. But now let the blow be delivered in a direction not at right angles, but oblique to the axis of the blade. The angle of resistance will then be given by an oblique section of the blade, and in proportion to the obliquity it will be finer than the angle of a straight cross section. It is on exactly the same principle that the steepness of a road or path on a mountain side is diminished by giving it a zigzag course. With a straight edge this effect can be produced by what is called a drawing cut. But it is far more simply and certainly produced by giving a permanent curvature to the edge in the part where the stroke falls. A weapon thus formed cannot help presenting an oblique section of the blade in the act of cutting, and therefore will cut better than a straight weapon of similar transverse section. This is the principle of all curved swords, exemplified in the choice Persian blades, in the common Indian sabre (Talwár), and in the light cavalry sword of almost identical pattern which was used in our own service in the Peninsular and Waterloo campaigns. Near the hilt the blade is nearly straight, but towards the centre of percussion it bends rapidly away. This effect is enhanced by mounting the sword so that the initial direction of the blade, from which the curve falls back, makes a sensible angle with the line of direction of the hilt, and goes before it to meet the object struck at; in the sword-smith's terms, by making the edge "lead forward." Hence the elegant double curve made by the blade and hilt of the Persian sabre. The same rule is followed, though less obviously, by the most recent European patterns. In Japanese swords it is reversed, for some reason which I have never seen explained.

The use of a curved blade is of unknown antiquity in the East. Its most ancient form was probably short, and broader at the point

than at the handle (the scimitar properly so called); an exaggerated representation of this type is the conventional weapon of Orientals and barbarians among the painters of the Renaissance or even later. Passing over earlier stages, however, let us come to the sabre which was made known to Western Europe by the crusades, and whose form and fashion have continued to our own day without notable change. These Indian and Persian arms exhibit the perfection of a specialised type. Great cutting power is gained by the curvature, which ensures an oblique section of the blade, and therefore an acuter angle of resistance, being presented to the object struck. Everything else is sacrificed to the power of the edge, and sacrificed deliberately. The small grip and the partial or total neglect of protection for the sword-hand are part of the same plan. Defence is left to the shield and armour. The curious projecting pommel of the commonest pattern of Indian sabre may act, indeed, as a guard for the wrist, but it has other uses; it may become a weapon of offence at close quarters, it balances the weight of the blade, and it may be grasped with the left hand for a two-handed blow. Scottish broadswords not uncommonly have a kind of outside loop made in the hilt for the same purpose.

More time and labour have been given to the making and adornment of choice weapons in Syria, Persia, and India than in any other part of the world. The best steel always came, it appears, from India. Damascus has given its name to the characteristic processes of Oriental metal-work, but has long ceased to be the chief seat of the art: "the best blades at the present day are still made in Khorassan, where the manufacture has been carried on since the time of Timour, who transported thither the best artificers of Damascus."* Nevertheless, Damascus blades, or what purport to be such, are still freely sold to travellers in the East. One such purchaser, I am told, observed that a number of these swords had the same inscription in Arabic characters. He was unable to read it himself, but afterwards consulted an Orientalist, who informed him that the writing signified—"I am *not* a Damascus blade." It may be believed that the interpretation was faithful, for the jest is quite in the Persian manner. The damasked or "watered" appearance of the blades which are most highly esteemed in the East appears to have been originally due to an accidental crystallisation of the steel in the process of conversion. The production of it was long thought a secret, but Western experts have now both explained and imitated it.†

While we are among Indian weapons, we may learn from them that the development of the sword from the dagger by successive steps and modifications is not a matter of mere archæological conjecture. Almost conclusive proof is given by the series of intermediate forms between the straight broad dagger (Katár), with a handle formed by a pair of cross-bars set close together between two

* Egerton, 'Handbook of Indian Arms,' p. 56.

† Wilkinson, 'Engines of War' (1841), pp. 200 *et seq.*

other bars parallel to the axis of the blade which serve as hand-guards, and the long sword with gauntlet hilt called Paṭá. The dagger, as far as the blade goes, is of a widespread type: the mediæval short swords, for example, called by modern antiquaries "anelace" or "langue-de-bœuf" (though there is some doubt as to what anelace or anlas, a name peculiar to England and of unknown origin, really means), are not unlike it. But the mounting is peculiar, and enables us to follow the transitions. First the blade is made about a third or a half longer. Then a kind of shell covering the back of the hand is added to the bars of the hand-guard. In this form the weapon is called "Bara jamdádú" (death-giver), and seems to be known only in a limited part of Southern India. Finally the blade is lengthened into a double-edged sword, and the hand-guard is closed in so as to make a complete gauntlet-shaped hilt. The original cross-bar handle remains, making the grip entirely different from that of an ordinary sword.* One does not see how an arm thus mounted can be used except for a sweeping blow, no room being given for the slightest play of the wrist. It is not uncommon to find old Spanish or other European blades mounted in these gauntlet hilts—a fact worth noticing, to correct the popular impression that Eastern swords are better than European ones. This is far from being generally true. Not only may old Spanish, Italian, or German blades be found in collections of Oriental arms, but in quite modern times Indian horsemen have been known to use by preference English light cavalry swords, remounted in their own fashion, and to do terrible execution with them. European swords have been found ineffective in Indian warfare, not because they were bad in themselves, but because they were not kept sharp like the Indian ones. "A sharp sword will cut in any one's hand," said an old native trooper to Captain Nolan in answer to questions as to the secret of the Indian horsemen's blows. And if European sword-smiths do not produce habitually such elaborate work as those of Persia and Damascus, it is not because they have not the secret of their Eastern fellow-craftsmen, but because the time and expense required for watered blades are such as would not be compensated by the price obtainable in the Western market. Only in the East, where men seem to take no count of time, and where centuries have passed without historians and without any means of fixing dates, could this branch of the armourer's art have arisen, or be regularly practised.

Similarly, we have all read in Walter Scott's 'Talisman' the spirited (though, it must be confessed, inaccurate †) description of the sword-

* Examples of all the stages may be seen in the Indian section of the South Kensington Museum, or still better in the Pitt-Rivers collection, where a case is specially arranged to show the transition.

† Richard I. is made to wield a two-handed sword, a weapon unknown in his time, and used only by foot-soldiers when it did come in some three centuries later; and Saladin's is described as having a *narrow* curved blade, whereas Indo-Persian sabres are, on the average, broader if anything than European swords.

feats performed by Richard and Saladin; and most readers probably imagine the cutting of the cushion and the veil to require some temper to be found only in Oriental blades, or some refinement of address peculiar to Oriental hands. But these and other feats of Eastern swordsmen have been and are repeated with success by Europeans in our own time. It is true that a light and very sharp sword, not the service arm, is used for that special purpose.

Various peculiar types of curved swords and more or less similar weapons occur in different parts of the East. One which deserves special mention, from the distances to which it has travelled, is the yataghan type. The doubly-curved blade of the yataghan, still a constant part of the armed Albanian's equipment, and a favourite Turkish weapon,* is identical in form with the short sword or falchion (Kopis) figured on sundry Greek monuments, and with the Kukri of Nepal. This last, indeed, is commonly broader and more curved; but there is an elongated variety of it which cannot be distinguished from the yataghan, and which occurs in Nepal itself, in the Deccan, and in Sind. A precisely similar arm, probably imported by Roman auxiliaries, has been found at Cordova and elsewhere in Spain, and may be seen in the Pitt-Rivers collection and the Musée d'Artillerie. It makes a very handy and formidable weapon, combining, if not too much curved, a strong cutting edge with considerable thrusting power. Of its birthplace, I believe, nothing is known; it is more or less used in all the Mahometan parts of Asia, and the geographical distribution would point to Persia or thereabouts for a common origin; but then Persia is just the country where the thing seems to be least common, and the word is purely Turkish. It is not impossible that, notwithstanding the strong temptation to make out a pedigree, we have here a case of independent invention in two or more distinct quarters; and in fact the Kukri of the Gorkhas is stated (on what authority I do not know) to be derived from a bill-hook used for woodcutter's work in the jungles. In modern times the yataghan has been the parent of the French sword-bayonet, and it was even proposed by Colonel Marey, the author of a full and ingenious monograph on the forms and qualities of swords, to make the infantry officer's sword of this pattern.

There are many kinds of outlandish weapons, in Nepal and farther east, of which the edge has a concave instead of a convex curvature. I doubt whether these be properly swords; at all events, they have had no influence on European forms. The Japanese swords also stand by themselves, though they are historically nothing but a superior variety of the general type which is found in China and Burmah, and to some extent in the Malay archipelago. They are exceedingly sharp, but have no flexibility at all. It may be worth noting that the custom of wearing two swords, which has been the

---

* I do not think it was adopted by the Greeks. In the Klephtic ballads it seems to be opposed, as the Turkish arm, to the Greek sword (σπαθί).

occasion of some curiosity and conjectural explanation, is not confined to Japan. Certain Arabs in the Mahratta service are stated to have done the same.* The two swords of the Japanese, however, are of such different sizes as to be rather comparable to the sword and dagger of Europeans, and perhaps there is really nothing to explain.

We pass now to the other special line of development, that of the rapier and small-sword. Whatever differences of opinion may be possible about the sabre, there can be no doubt that the straight sword which ultimately became a thrusting sword is an extension of the dagger. The East is rich in daggers of many forms, so rich that in India alone a score of distinct names for distinct varieties of the weapon appear to be current. There is a broad difference, however, between the straight and the curved daggers, and the modes of using them; the straight ones being held like a sword, the curved ones the reverse way, with the little finger next the blade. Among the curved species is one of which the shape would be puzzling if it were not known to be simply copied from a buffalo horn. The proof is that a dagger of this class is sometimes nothing but the split and sharpened buffalo horn itself. I am not sure that all the curved daggers may not be due to some imitation of this kind, and thus be quite unconnected with the course of development leading up to the modern sword. That the curved sabre is modified from a straight sword, not enlarged from a curved dagger, is, I think, too plain for discussion. The broad-bladed straight dagger which lengthened into the gauntlet-hilted sword has already been mentioned. But neither in this nor in any other case does the enlargement of the dagger appear to have suggested in the East the fabrication or use of a full-sized sword with thrusting for its chief or sole purpose. The rapier, the duelling sword, and the art of fencing, are purely Western inventions. Before going further, let us put a needful distinction of terms beyond mistake. A duelling sword and a rapier are not the same thing, though they are often confused. The rapier is a cut-and-thrust sword so far modified as to be used chiefly for pointing, but not to the complete exclusion of the edge. The duelling sword is a weapon made, and capable of being used, for pointing only. Such a construction would be naturally first applied to the dagger, as its cutting edges could never be of much offensive service unless it were of a large and clumsy type. Cutting power being once regarded as secondary or superfluous, the two-edged blade is narrowed for convenience of carriage, perhaps also of concealment, until thickening becomes necessary to make it strong enough. This reinforcement may be effected by a ridge on either side of the blade, or by a ridge on one side only, which soon becomes as much or as little of an edge as the original and now degraded edges of the blade. From the narrow two-edged blade strengthened by a single "median ridge" we get a purely thrusting blade of triangular section, or an approximately bayonet-

* Egerton, *op. cit.*, p. 114.

shaped blade as we should now call it. From the blade with a double "median ridge" we get a blade of quadrangular section, not corresponding to anything now in familiar use. Both the three-edged and the four-edged shape occur among mediæval daggers; they are also found, though exceptionally, in Indian specimens. It is difficult to say when they were introduced. We have a distinct record of three-edged swords or long daggers having been employed at the battle of Bovines (A.D. 1214); they are specially described by the chronicler as a novelty.* But no example of so early a date appears to be either preserved or figured anywhere; and it was as nearly as possible five centuries afterwards that the bayonet-shaped small-sword prevailed over the rapier. It is worth noticing that some of the Scottish broadswords of the late seventeenth and early eighteenth centuries have a "median ridge" so strongly marked as to make them almost three-edged.

As for the two-edged rapier, its parentage is obvious. It is the military sword of all work, in the form it had assumed in the first half of the sixteenth century, lengthened, narrowed, and more finely pointed.† The interesting question is, what led to the use of the point being studied and developed at that particular time. It may seem a paradox to say that the art of fencing is due to the invention of gunpowder; but I believe it to be true. So long as the body was protected by armour, there was no necessity and no scope for fine swordsmanship. Hard hitting was the only kind of attack worth cultivating. Fire-arms, however, made armour not only of less value, but at short ranges a source of positive danger, just as nowadays, when the side of an ironclad is once penetrated by shot, the splinters make matters worse than if there had been no resistance at all. Armour being abandoned as worse than useless against fire-arms, it became needful to resort to skill instead of mechanical protection for defence against cold steel at close quarters. Various experiments were tried; the shield was reduced in dimensions to make it more manageable, and in England sword and buckler play, which had long been a favourite national pastime, still had, at the very end of the sixteenth century, its zealous advocates against the new-fangled rapier. But the point, of no avail against complete armour, soon manifested its superior power when this barrier was removed. There is some ob-

* 'Guillelmi Armorici liber' (Guillaume le Breton), anno 1214, § 192 (p. 283 of ed. 1882, published by the Société de l'histoire de France).—" . . . Ante oculos ipsius regis occiditur Stephanus de Longo Campo, miles probus et fidei integre, cultello recepto in capite per ocularium galee. Hostes enim quodam genere armorum utebantur admirabili et hactenus inaudito; habebant enim cultellos longos, graciles, *triacumines, quolibet acumine indifferenter secantes a cuspide usque ad manubrium*, quibus utebantur pro gladiis. Sed per Dei adjutorium prevaluerunt gladii Francorum," &c.

† It has been said that the rapier and its distinctive manner of use were derived from an elongated dagger employed for piercing the joints of plate armour; but I have met with nothing to support this view.

scurity about the local origin of the rapier and of fencing. A credible tradition refers it to Spain, whence it was imported into Italy by the Spanish armies early in the sixteenth century. The finest old rapiers are Spanish, and there is mention of very early Spanish books on the subject, which, however, do not seem to be extant.*

From Italy the fashion came into France and England, and spread apace, not without grumbling from the older sort of gentlemen and soldiers, of which the echoes are yet audible to us in sundry passages of Shakespeare. At some time between 1570 and 1580 the rapier became the favourite companion of the exquisites of London. "Shortly after (the twelfth or thirteenth year of Queen Elizabeth)," says Howes, the continuer of Stow's 'Annals,' "began long tucks, and long rapiers, and he was held the greatest gallant, that had the deepest ruff and longest rapier: the offence to the eye of the one, and the hurt unto the life of the subject that came by the other, caused her Majesty to make proclamation against them both, and to place selected grave citizens at every gate to cut the ruffs and break the rapiers' points of all passengers that exceeded a yard in length of their rapiers, and a nail of a yard in depth of their ruffs." A later writer fixes the date of this proclamation to 1586, and adds that it forbad rapiers to be "carried, as they had been before, upwards in a hectoring manner," but says nothing of the ruffs.† In 1594–5 two English treatises appeared on the new art of fence, one translated from the Italian of Giacomo di Grassi, the other the work of Vincentio Saviolo,‡ an Italian master established in England. The translator of Grassi tells us in his "Advertisement to the Reader," that "the sword and buckler fight was long while allowed in England (and yet practice in all sorts of weapons is praiseworthy), but now being laid down, the sword, but with serving-men, is not much regarded § and the

---

* See Nicolao Antonio, 'Bibl. Hispana Vetus,' tom. 2, p. 305, and 'Bibl. Hispana Nova,' tom. 1, p. 468, and tom. 2, p. 57, who names two Spanish authors, Jacobus or Jaume Pons (or Pona) of Perpignan, and Petrus de Turri, as having written in 1474. He does not profess to have seen their books, but gives as his authority a work of Luis Pacheco de Narvaez ('Engaño y desengaño de los errores, que se an querido introducir en la destreza de las armas,' Madrid, 1635), which I have not been able to consult. The same names are given by Morsicato Pallavicini, a Sicilian author of the late seventeenth century, but without any reference.

† Stow, 'Annals,' continued by Edmond Howes, Lond. 1614, p. 869; 'Survey of London,' ed. 1755, vol. ii. p. 543 (in Strype's additional matter). Such a proclamation was, according to modern ideas, quite illegal: but much else of the same kind was acquiesced in all through Elizabeth's reign.

‡ There is a second book of this treatise with a separate title-page, 'Of honor and honorable quarrels,' supposed by Warburton to be alluded to in Touchstone's exposition of the lie seven times removed. I cannot think this at all certain; the coincidence of matter is not very close, and it appears from Saviolo that other books of the kind were in existence.

§ Cf. Florio, 'First Fruits' (1578), cited by Malone on 'King Henry IV., Part I., act i. sc. 3, where the buckler is called "a clownish, dastardly weapon, and not fit for a gentleman."

rapier fight generally allowed, as a weapon because most perilous, therefore most feared, and thereupon private quarrels and common frays most shunned." On the other hand, some partisans of the old sword and buckler play maintained its excellence on the express ground that men skilled in it might fight as long as they pleased without hurting one another; and others denounced the rapier as "that mischievous and imperfect weapon which serves to kill our friends in peace, but cannot much hurt our foes in war" (George Silver, 'Paradoxes of Defence,' 1599). But they were soon discomfited. In 1617 we find one Joseph Swetnam, a garrulous and not original author, declaring that the short sword or back-sword (a stout sword so called from having only one edge) is against the rapier "little better than a tobacco pipe or a fox tail." We must not suppose that the rapier fight of the sixteenth century resembled modern fencing. It was the commoner practice to hold a dagger in the left hand for parrying; this, by the way, has an odd analogy in China, where instruments like blunt skewers are used for the same purpose. And not only did the use of the dagger, or in its absence of the gauntleted left hand, make the conditions different from those of the modern fencing-school, but the principles and methods were as yet crude and unformed. The fencing-match in 'Hamlet' is now presented according to the modern fashion, and Dumas and Gautier, both of whom knew the historic truth well enough, freely introduce the modern terms and rules into the single combats of their novels. In each case this course is justified by artistic necessity. But if we look to the engravings in Saviolo or Grassi, we shall find that Hamlet and Laertes, when the play was a novelty at the Globe Theatre, stood at what would now be thought an absurdly short distance (for the lunge, or delivery of the thrust by a swift forward movement of the right foot and body, with the left foot as a fixed point, was not yet invented), with their sword-hands down at their knees, the points of their rapiers directed not to the breast but to the face of the adversary, and their left hands held up in front of the shoulder in a singularly awkward attitude. A great object was to seize the adversary's sword-hilt with the left hand; and this perhaps explains the "scuffling" in which Hamlet and Laertes change foils—a thing barely possible in a fencing-match of the present day. An incidental illustration of the part of the left hand in defence is given in 'Romeo and Juliet,' where it is related that Mercutio

> "with one hand beats
> Cold death aside, and with the other sends
> It back to Tybalt."

The duel with rapier and dagger had particular rules of its own; and the handling of a "case of rapiers" (that is, a rapier in either hand) was also taught, but, one would think, only for display.

During this period the use of the edge was combined with that of the point, but the point was preferred. "To tell the truth," says

Saviolo. "I would not advise any friend of mine, if he were to fight for his credit and life, to strike neither mandrittas nor riversas" (the technical names of direct and back-handed cuts), "because he puts himself in danger of his life; for to use the point is more ready, and spends not the like time." In the books of the seventeenth century the instructions for mandrittas and riversas disappear accordingly, and at the beginning of the eighteenth we find the small-sword in existence and the rapier gradually giving place to it. Experiments had already been made with thrusting blades of triangular or quadrangular section: at least, specimens of such, ascribed to the early seventeenth or even the end of the sixteenth century, may be seen in museums. In some of these cases, however, one would like to ascertain that a more recent blade has not been mounted in a hilt of the period attributed to the weapon. Be that as it may, the small-sword completely prevailed over the two-edged rapier some time about 1715. At the same time that the form of the blade was changed, its length, which had been excessive, was reduced to a handier and not less effective compass. A sword 36 inches long was reckoned short at the beginning of the seventeenth century, and some rapiers extend to four feet and more. The standard length of the modern small-sword and its representative for fencing purposes, the foil, is from 32 to 34 inches only. Sir William Hope, of Edinburgh, writing in 1692, considers three-quarters of an ell to be "an indifferent good length," that is, "neither too long, which would be unhandsome (i. e. unhandy or clumsy), nor too short, which would be very inconvenient": taking the ell at 45 inches, this comes very near the present measure. As regards the mounting and guard also, there was a marked return to simplicity. The elaborate work of the Spanish rapier-hilts disappears, to be replaced by a plain shell guard for the duelling sword, and a very light hilt, capable, however, of much decoration if desired, for the walking-sword which every gentleman habitually wore until near the end of the last century. Meanwhile the art of fencing made rapid progress, and may be said to have been fixed in substance upon its modern lines by 1750 or thereabouts. To give an account of its development before and since that time would require not a part of a discourse, nor a whole discourse, but a book. Such a book, strange to say, does not yet exist, not even in France, the chief seat of the art ever since the first half of the seventeenth century, when the supremacy passed to her from Italy. The lunge had, indeed, been taught and figured by Italian masters; but the riposte, which is the very life of modern fencing as a system of combined defence and offence, is undoubtedly a French invention. All the modern authorities of much value are either French or openly founded on the French school; there exists, however, a distinct Italian school, which still keeps up a shadow of the older rapier play.

One is tempted in the various forms and uses of the sword to see a reflection of the general temper, and even the tastes and style of the age. The sword of each period seems fitted by no mere accident to

the gentlemen, both scholars and soldiers, like Bassanio, who wore and handled it. The long rapier, with its quillons and cunningly wrought metal-work, and somewhat rigid hand-hold, is a kind of visible image of the stately and involved periods of Elizabethan prose. I can persuade myself that it was not in the nature of things for Sidney or Raleigh to be otherwise armed. When we come to the great forerunners of modern English, Hobbes (who has in nowise forgotten to put a sword in the right hand of the mystical figure representing the might of the State in the frontispiece to his 'Leviathan') seems to wield an Andrea Ferara, such a blade and so mounted as Cromwell's, dealing nimbly and shrewdly with both edge and point. And in the exquisite dialectic of Berkeley and Hume, as clear and graceful as it is subtle, and without a superfluous word, we surely have the true counterpart of the finished play of the small-sword, the perfection of single combat. Warfare is on a grander scale now, the controversies of philosophers as well as the campaigns of generals. There are modern philosophical arguments which profess to be more weighty, as they are certainly more voluminous, than Hume's or Berkeley's, and which remind one not of an assault between two strong and supple fencers in which every movement can be followed, but of a modern field-day, where there is much hurrying to and fro, much din, dust, and smoke, and extreme difficulty in discovering what is really going on.

But our story is not fully done. At the same time, or almost the same time, with the small-sword there came in an offshoot of this class of weapons which has a curious little history of its own, namely the bayonet, a modified dagger in its immediate origin, but influenced in its settled ordinary form by the small-sword, and by the sabre and yataghan in various experimental forms which have ended in the sword-bayonet largely used in Continental services, and to some extent in our own. There is a new French pattern of this weapon in which the yataghan curve is abandoned; though quite straight, it still has only one edge. It seems a considerable improvement on the shape which we have copied from an older French model. There have been some rather pretentious writings in France and elsewhere about the reduction of bayonet practice to a system; I am inclined to think that a man who knows how to use the point of a sword (the necessary foundation of all skill in hand-weapons) will very soon learn what the bayonet is and is not capable of.

A word is also due to the modern military sabre. This, broadly speaking, is a continuation of the straight European military sword of the sixteenth century, lengthened and lightened after the example of the rapier, but one-edged instead of two-edged (which, according to the French authorities, is the decisive mark of *sabre* as distinguished from *épée*), and in many cases more or less curved after the fashion of the Eastern swords. Meanwhile, the long straight sword has thrown out a most eccentric development, or even "sport," in the shape of the German Schläger with which students' duels are fought. This

is too remotely connected with the main part of the subject to be dwelt upon here; the duels in question, for the rest, have been often and pretty recently described by English observers. The rapier and the small-sword are weapons of single combat, not of general military use; the small-sword is too fragile, the rapier both too fragile and too long, for a soldier's convenience. It is true that it was proposed by no less an authority than Marshal Saxe to arm cavalry with long bayonet-shaped swords, and his opinion has been followed by at least one modern writer. But it is founded on the erroneous notion that a good cutting sabre cannot have a good point, and therefore either the edge or the point must be wholly sacrificed: a notion which has so far prevailed that late in the eighteenth century an excessively curved light cavalry sabre (apparently copied with close fidelity from an Indian model) was introduced throughout the armies of Europe. It was the weapon of our light dragoons all through the Peninsular and Waterloo campaigns, and effective for cutting, but almost or quite useless for pointing. Even now there remains a certain difference in most services between the shape of the light and the heavy cavalry swords, the heavy cavalry sword being straighter, or sometimes perfectly straight. But it is pretty well understood by this time that one and the same sword can be made, though not so perfect for thrusting as the duelling sword, nor so powerful for cutting as an Indian talwár or the old dragoon sabre, yet a very sufficient weapon for both purposes. A blade of moderate length, not too broad, and lightened by one or more grooves running nearly from hilt to point, may be shaped with a curve too slight to interfere gravely with the use of the point, yet sensible enough to make a difference in favour of the edge. This plan is now generally followed.

The use of the edge, after being unduly neglected in consequence of the startling effectiveness of the rapier-point, has also been more carefully studied in modern times. Closely connected with the error just now mentioned, that the same blade cannot be good for both cutting or thrusting, is an equally erroneous belief that a cut cannot be delivered with sufficient force except by exposing one's whole body. The old masters of rapier-fence already knew better. What says Grassi in the contemporary English version? "By my counsel he that would deliver an edge-blow shall fetch no compass with his shoulder, because whilst he beareth his sword far off, he giveth time to the wary enemy to enter first; but he shall only use the compass of the elbow and the wrist: which, as they be most swift, so are they strong enough if they be orderly handled." This is exactly what the best modern teachers say. Though sabre-play cannot rival the refinements of the lighter and more subtle small-sword, there is much more science in it than would be supposed by any one not acquainted with the matter; and it may easily be seen that a pair of single-stick players who have learnt from a good master do, in fact, expose themselves wonderfully little. Nor is it easy to say on which side the advantage ought to be in a combat between foil and sabre.

the players being of fairly equal skill, and each acquainted with the use of both weapons.

My final word, albeit it savour of egotism, shall be one of practical testimony and counsel to a generation of students. I must add my voice to those of a long chain of authorities, medical and other, to bear witness that the exercise of arms, whether in the school of the small-sword, or in the practice, more congenial, perhaps, to the English nature, of the sturdier sabre, is the most admirable of regular correctives for the ill habits of a sedentary life. It is as true now as when George Silver wrote it under Queen Elizabeth, that "the exercising of weapons putteth away aches, griefs, and diseases, it increaseth strength and sharpeneth the wits, it giveth a perfect judgment, it expelleth melancholy, choleric and evil conceits, it keepeth a man in breath, perfect health, and long life."

[F. P.]

GENERAL MONTHLY MEETING,

Monday, June 4, 1883.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

George Claudius Ash, Esq.
Henry Swainson Cowper, Esq.

were elected Members of the Royal Institution.

The Managers reported that they had re-appointed Professor James Dewar, M.A. F.R.S. as Fullerian Professor of Chemistry.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Lists of the Antiquarian Remains in Madras. By R. Sewell. Vol. I. 4to. 1882.
Account of the Great Trigonometrical Survey of India. Vols. VII. and VIII. 4to. 1882.
Accademia dei Lincei, Reale, Roma—Atti, Serie Terza. Vol. VII. Fasc. 7, 8, 9, 10. 4to. 1883.
Agricultural Society of England, Royal—Journal, Second Series, Vol. XIX. Part 1. 8vo. 1883.
Antiquaries, Society of—Proceedings, Second Series, Vol. IX. No. 1. 8vo. 1883.
Asiatic Society, Royal—Journal, Vol. XV. Part 2. 8vo. 1883.
Asiatic Society of Bengal—Proceedings, No. 1. 8vo. 1883.
Astronomical Society, Royal—Monthly Notices, Vol. XLIII. No. 6. 8vo. 1883.
Ateneo Veneto—Rivista Mensile. Serie IV. Nos. 1, 5, 6, 7; Serie V. VI. and VII. Nos. 1, 2, 3. 8vo. Venezia. 1881–3.
Bankers, Institute of—Journal, Vol. IV. Part 6. 8vo. 1883.
British Architects, Royal Institute of—Proceedings, 1882–3, Nos. 14, 15. 4to.
Chemical Society—Journal for May, 1883. 8vo.
Editors—American Journal of Science for May, 1883. 8vo.
Analyst for May, 1883. 8vo.
Athenæum for May, 1883. 4to.
Chemical News for May, 1883. 4to.
Engineer for May, 1883. fol.
Horological Journal for May, 1883. 8vo.
Iron for May, 1883. 4to.
Nature for May, 1883. 4to.
Revue Scientifique and Revue Politique et Littéraire for May, 1883. 4to.
Telegraphic Journal for May, 1883. fol.
Franklin Institute—Journal, No. 689. 8vo. 1883.
Geographical Society, Royal—Proceedings, New Series, Vol. V. No. 5. 8vo. 1883.
Geological Institute, Imperial, Vienna—Jahrbuch, Band XXXIII. No. 1. 8vo. 1883.
Verhandlungen, Nos. 1–6. 8vo. 1883.

Geological Society—Abstracts of Proceedings, 1882–3, Nos. 438, 439. 8vo.
Quarterly Journal, No. 154. 8vo. 1883.
Gesellschaft der Arzte, Wien—Medizinische Jährbücher. Heft 1. 8vo. 1883.
Johns Hopkins University—American Journal of Philology, No. 13. 8vo. 1883.
American Chemical Journal, Vol. V. No. 1. 8vo. 1883.
University Circulars, No. 22. 4to. 1883.
Lisbon, Sociedade de Geographia—Bulletin, 3e Serie, No. 8. 8vo. 1882.
Manchester Geological Society—Transactions, Vol. XVII. Part 7. 8vo. 1883.
Meteorological Office—Communications from the International Polar Commission, Part 4. 4to. 1883.
Quarterly Weather Report, 1880. fol. 1883.
Rainfall Tables of the British Isles for 1866–1880. Compiled by G. J. Symons. 8vo. 1883.
Meteorological Society—Instructions for the Observation of Phenological Phenomena. 8vo. 1883.
Morris, Rev. F. O., B.A. (the Author)—All the Articles of the Darwin Faith. 8vo. 1882.
Numismatic Society—Chronicle and Journal, 1883, Part 1. 8vo.
Pharmaceutical Society of Great Britain—Journal May, 1883. 8vo.
Photographic Society—Journal, New Series, Vol. VII. Nos. 7, 8. 8vo. 1883.
Physical Society of London—Proceedings, Vol. V. Part 3. 8vo. 1883.
Rodwell, G. F. Esq. (the Author)—Effects of Heat on Certain Haloid Compounds of Silver, &c. (Phil. Trans.) 4to. 1882.
Royal Society—Catalogue of Scientific Books in the Library. Parts 1 and 2. 8vo. 1881–3.
Sanitary Institute of Great Britain—Transactions, Vol. IV. 8vo. 1883.
Seismological Society of Japan—Transactions, Vol. V. 8vo. 1883.
Society of Arts—Journal, May, 1883. 8vo.
Symons, G. J. Esq. F.R.S.—Monthly Meteorological Magazine, May, 1883. 8vo.
United Service Institution, Royal—Journal, No. 119. 8vo. 1883.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1883: Heft 4. 4to.
Victoria Institute—Journal, Nos. 64, 65. 8vo. 1883.
Wood, H. T. Esq. B.A. (the Author)—The Government Patent Bill. (Journal of Society of Arts). 8vo. 1883.
Yorkshire Archæological and Topographical Association—Journal, Part 29. 8vo. 1883.

WEEKLY EVENING MEETING,

Friday, June 8, 1883.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Professor Dewar, M.A. F.R.S. *M.R.I.*

The Electric Arc and Chemical Synthesis.

(Abstract deferred.)

GENERAL MONTHLY MEETING,

Monday, July 2, 1883.

Sir Frederick Pollock, Bart. M.A. Manager and Vice-President, in the Chair.

J. G. Crawford, Esq.
Gustavus Steinthal, Esq.

were elected Members of the Royal Institution.

The decease of Mr. William Spottiswoode, Pres. R.S. Manager and Vice-President, on June 27th, was announced from the Chair.

In conformity with Resolutions passed by the Managers and Members, a Sub-Committee of Managers met this day and passed the following Resolutions:—

Resolved: "That the Managers of the Royal Institution desire to record their profound regret for the recent death of Mr. William Spottiswoode, who for so many years, as Manager, Treasurer, or Secretary, rendered such eminent services to the Royal Institution.

"In him the world has lost a man of the first distinction. While his own researches have done much to advance the progress of mathematical and physical science, his encouragement and wise counsels to other workers have also been of the utmost value. As President of the Royal Society he has fulfilled the duties of his high office with conspicuous ability and success.

"As one of the heads of the important public establishment, with which his family has been long connected, it is known that he afforded an admirable example of what should be the conduct of one placed in a position of so much responsibility and authority.

"His interest in all that tends to promote the general welfare of humanity was equally remarkable. No good work of a public nature, which came within the range of his means, was ever allowed to proceed without his aid and sympathy.

"His character presented a remarkable combination of intellectual strength and fine moral qualities. His judgment was unerring, and of so discreet a kind, as to ensure for it a deference which was universal. In private life his kindliness, courtesy, and general culture, endeared him to his friends, and engaged the regard of all who were so fortunate as to become acquainted with him.

"The public tribute paid to the memory of the late President of the Royal Society, by his interment in Westminster Abbey, is a striking proof of the general esteem in which he was held.

"It rests for the Managers to express their deep sympathy with Mrs. Spottiswoode, and the other members of Mr. SPOTTISWOODE's family, under their heavy bereavement."

Resolved: "That a Copy of this Resolution be forwarded to Mrs. Spottiswoode."

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Geological Survey of India: Palæontologia Indica: Series X. Vol. II. Part 5. 4to. 1883.
The Lords of the Admiralty—Greenwich Observations for 1881. 4to. 1883.
Asiatic Society of Bengal—Proceedings. No. 2. 8vo. 1883.
Astronomical Society, Royal—Monthly Notices, Vol. XLIII. No. 7. 8vo. 1883.
Bankers, Institute of—Journal, Vol. IV. Part 7. 8vo. 1883.
British Architects, Royal Institute of—Proceedings, 1882-3, No. 16. 4to.
Chemical Society—Journal for June, 1883. 8vo.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Series II. Vol. III. Part 3. 8vo. 1883.
Editors—American Journal of Science for June, 1883. 8vo.
Analyst for June, 1883. 8vo.
Athenæum for June, 1883. 4to.
Chemical News for June, 1883. 4to.
Engineer for June, 1883. fol.
Horological Journal for June, 1883. 8vo.
Iron for June, 1883. 4to.
Nature for June, 1883. 4to.
Revue Scientifique and Revue Politique et Littéraire for June, 1883. 4to.
Telegraphic Journal for June, 1883. fol.
Franklin Institute—Journal, No. 690. 8vo. 1883.
Geographical Society, Royal—Proceedings, New Series, Vol. V. No. 6. 8vo. 1883.
Geological Society—Abstracts of Proceedings, 1882-3, Nos. 440, 441. 8vo.
Johns Hopkins University—American Chemical Journal, Vol. V. No. 2. 8vo. 1883.
Linnean Society—Journal, No. 128. 8vo. 1883.
Middlesex Hospital—Reports for 1880. 8vo. 1883.
Pharmaceutical Society of Great Britain—Journal, June, 1883. 8vo.
Preussische Akademie der Wissenschaften—Sitzungsberichte, I.-XXI. 4to. 1883.
Ramsay, A. Esq. F.G.S. (the Editor)—Scientific Roll, No. 11. 8vo. 1883.
Society of Arts—Journal, June, 1883. 8vo.
St. Pétersbourg, Académie des Sciences—Bulletins, Tome XXVIII. No. 3. 4to. 1883.
Symons, G. J. Esq. F.R.S.—Monthly Meteorological Magazine, June, 1883. 8vo.
Tasmania Royal Society—Report for 1881. 8vo. 1882.
Telegraph Engineers, Society of—Journal, Vol. XII. No. 48. 8vo. 1883.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1883: Heft 5. 4to.

GENERAL MONTHLY MEETING,

Monday, November 5, 1883.

Sir WILLIAM SIEMENS, D.C.L. LL.D. F.R.S. Manager and Vice-President, in the Chair.

John Coles, Esq.
Ludwig Mond, Esq. F.C.S.

were elected Members of the Royal Institution.

William Miller Ord, M.D. was elected a Manager in the room of the late Mr. William Spottiswoode, P.R.S.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Geological Survey of India: Records. Vol. XVI. Parts 2, 3. 8vo. 1883.
Memoirs. Vol. XXII. 8vo. 1883.
The Secretary of State for India—Account of the Great Trigonometrical Survey of India. Vol. IX. 4to. 1883.
Collection of Papers on Bee-keeping in India. fol. Calcutta, 1883.
The Trustees of the British Museum—Catalogue of Birds. Vols. VII. and VIII. 8vo. 1883.
The Meteorological Office—Quarterly Weather Report, 1877. Appendices and Plates. fol. 1883.
Hourly Readings, Part 4, 1882. 4to. 1883.
Academy of Natural Sciences, Philadelphia—Proceedings, 1882 and 1883, Part 1. 8vo. 1883.
Accademia dei Lincei, Reale, Roma—Memorie della Classe di Scienze Morali, Storiche e Filologiche. Serie 2d, Vol. VIII. 4to. 1883.
Memorie della Classe di Scienze Fisiche, Matematiche e Naturali. Vols. XI. XII. XIII. 4to. 1882–3.
Atti, Serie Terza: Transunti. Vol. VII. Fasc. 11, 12, 13, 14, 15. 4to. 1883.
Actuaries, Institute of—Journal. Nos. 128 (Index), 129, 130. 8vo. 1883.
American Philosophical Society—Proceedings, No. 112. 8vo. 1882.
Antiquaries, Society of—Proceedings, Second Series, Vol. IX. No. 2. 8vo. 1883.
Archæologia. Vol. XLVII. Part 2. 4to. 1883.
Asiatic Society, Royal—Journal, Vol. XV. Parts 3, 4. 8vo. 1883.
Asiatic Society of Bengal—Journal, Vol. LII. Part 1, No. 2. 8vo. 1883.
Proceedings, Nos. 3, 4, 5, 6. 8vo. 1883.
Astronomical Society, Royal—Monthly Notices, Vol. XLIII. No. 8. 8vo. 1883.
Australian Museum, Sydney—Report of the Trustees, 1882. fol. 1883.
Bagot, Alan, Esq. (the Author)—Life Brigades for Mining Districts. (K 105) 8vo. 1883.
Floods Prevention and Rivers Conservancy Bill. (K 105) 8vo. 1883.
Bankers, Institute of—Journal, Vol. IV. Part 8. 8vo. 1883.

Barnett, Samson, Esq. Jun.—Official Catalogue of the Engineering Exhibition, London. 8vo. July, 1883.
Batavia Observatory—Rainfall in the East Indian Archipelago, 1882. By J. P. Vander Stok, the Director. 8vo. Batavia, 1883.
Bavarian Academy of Sciences, Royal—Abhandlungen, Band XIV. 2te Abtheilung. 4to. 1883.
Meteorologische und Magnetische Beobachtungen bei Munchen, 1881 and 1882. 8vo. 1882–3.
Sitzungsberichte, 1883, Heft 1 and 2. 8vo. 1883.
Gedachtnissrede auf Otto Hesse. Von G. Bauer. 4to. 1882.
Belgique Académie des Sciences, &c.—Mémoires, Tomes XLIII. Partie 2, and XLIV. 4to. 1882.
Mémoires Couronnées, Tomes XLIV. and XLV. 4to. 1882–3.
————, Tomes XXXI. XXXIII. XXXIV. and XXXV. 8vo. 1881–3.
Bulletins, 3e Serie, Tomes I.-V. 8vo. 1881–3. Tables Generales, 2e Serie, Tomes XXI.-XL. (1867–80). 8vo. 1883.
Annuaires, 1882 and 1883. 16to.
Board of Trade (Standards Department)—Calculations of Densities and Expansions. fol. 1883.
British Architects, Royal Institute of—Transactions, 1882–3. 4to. 1883.
Proceedings, 1882–3, Nos. 17, 18; 1883–4, No. 1. 4to.
Canada Meteorological Office—Report, 1881. 8vo.
Chemical Society—Journal for July–Oct. 1883. 8vo.
Chief Signal Officer, U.S. Army—Annual Report, 1880. 2 Parts. 8vo. 1881.
Civil Engineers' Institution—Minutes of Proceedings, Vol. LXXII.-LXXIV. 8vo. 1883.
List of Members. 8vo. 1883.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Series II. Vol. III. Parts 4, 5. 8vo. 1883.
Dax: Société de Borda—Bulletins, 2e Serie Huitieme Année: Trimestre 2, 3. 8vo. 1883.
Department of the Interior, U.S.—Compendium of the Tenth Census (June 1, 1880). 2 vols. 8vo. Washington, 1883.
Devonshire Association for the Advancement of Science, Literature, and Art—Report and Transactions, Vol. XV. 8vo. 1883.
East India Association—Journal, Vol. XV. Nos. 2, 3, 4, 5. 8vo. 1883.
Editors—American Journal of Science for July–Oct. 1883. 8vo.
Analyst for July–Oct. 1883. 8vo.
Athenæum for July–Oct. 1883. 4to.
Chemical News for July–Oct. 1883. 4to.
Engineer for July–Oct. 1883. fol.
Horological Journal for July–Oct. 1883. 8vo.
Iron for July–Oct. 1883. 4to.
Nature for July–Oct. 1883. 4to.
Revue Scientifique and Revue Politique et Littéraire for July–Oct. 1883. 4to.
Steamship, Vol. I. Nos. 1 to 14. fol. 1883.
Telegraphic Journal for July–Oct. 1883. 8vo.
Ermacora, G. B. Esq. (the Author)—I Fenomeni Elettrostatica. Vol. I. 8vo. Padova, 1882.
Forster, Miss E. J. M.R.I.—The Chronicle of James I. King of Arragon. Trans. by John Forster. 2 vols. 8vo. 1883.
Franklin Institute—Journal, Nos. 691, 692, 693, 694. 8vo. 1883.
Geneva: Société de Physique et d'Histoire Naturelle—Mémoires, Tome XXVIII. Partie 1. 4to. 1882–3.
Geographical Society, Royal—Proceedings, New Series, Vol. V. Nos. 7 to 10. 8vo. 1883.
Geological Institute, Imperial, Vienna—Jahrbuch, Band XXXIII. Nos. 2, 3. 8vo. 1883.
Verhandlungen, Nos. 7–9. 8vo. 1883.

Geological Society—Abstracts of Proceedings, 1882–3, Nos. 440, 441. 8vo.
Quarterly Journal, No. 155. 8vo. 1883.
Geological Society of Ireland—Journal. Vol. XVI. Part 2. 8vo. 1882.
Gladstone, J. H. Esq. Ph.D. F.R.S. M.R.I. and A. Tribe, Esq. (the Authors)—The Chemistry of the Secondary Batteries of Planté and Faure. (Nature Series.) 16mo. 1883.
Gore, G. Esq. LL.D. F.R.S. (the Author)—The Scientific Basis of National Progress. 8vo. 1882.
Iron and Steel Institute—Journal for 1883, No. 1. 8vo. 1883.
Johns Hopkins University—American Journal of Philology, No. 14. 8vo. 1883.
American Chemical Journal, Vol. V. Nos. 3, 4. 8vo. 1883.
University Circulars, Nos. 24, 25. 4to. 1883.
Leeds Philosophical and Literary Society—Report, 1882–3. 8vo.
Linnean Society—Journal, Nos. 99, 100, 129. 8vo. 1883.
Lisbon, Sociedade de Geographia—Bulletin, 3e Serie, Nos. 11, 12; and 4e Serie, No. 1. 8vo. 1883.
Expedição Scientifica á Serra da Estrella em 1881. 4°. 1883.
Lunacy Commissioners—Thirty-seventh Report. 8vo. 1883.
Manchester Geological Society—Transactions, Vol. XVII. Parts 8, 9. 8vo. 1883.
Manchester Steam Users' Association—Reports, 1880, 1881, and 1882. 8vo.
Maryland Medical and Chirurgical Faculty—Transactions, 85th Session. 8vo. 1883.
Massaroli, Guiseppe, Esq. (the Author)—Phul e Tuklatpalasar II. Salmanasar V. E. Sargon Questioni Biblico Assire. 8vo. Roma, 1882.
Mayall, J. E. Esq. F.C.S. M.R.I.—Transactions of the Brighton Health Congress. 8vo. 1881.
Mechanical Engineers' Institution—Proceedings, Nos. 2, 3. 8vo. 1883.
Medical and Chirurgical Society—Proceedings, New Series, No. 3. 8vo. 1883.
Meteorological Society—Quarterly Journal, Nos. 46, 47. 8vo. 1883.
Meteorological Record, No. 9. 8vo. 1883.
Montpellier Académie des Sciences et des Lettres—Mémoires, Tome X. Fasc. 2. 4to. 1883.
Musical Association—Proceedings, 1882–3. 8vo. 1883.
National Association for Social Science—Proceedings, Vol. XVI. No. 5. 8vo. 1883.
Norway Royal University, Christiania—Jahrbuch des Norwegischen Meteorologischen Instituts, 1877–1881. 4to. 1879–82.
H. Siebke and J. S. Schneider. Enumeratio Insectorum Norwegicorum. Fasc. 5. 8vo. 1880.
C. M. Guldberg and H. Mohn. Études sur les Mouvements de l'Atmosphere. 2e Partie. 4to. 1880.
H. H. Reusch. Silurfossiler og Pressede Konglomerater I Bergensskifrene. fol. 1882.
T. Hiortdahl. Krystallographisk-Chemiske Undersogelier. 4to. 1881.
L. B. Stenersen. Myntfundet fra Graeslid I Thydalen. 4to. 1881.
G. O. Sars. Application of Autography in Zoology. 8vo. 1882.
Numismatic Society—Chronicle and Journal, 1883, Part 2. 8vo.
Owen, Richard, C.B. F.R.S. F.L.S. &c. (the Author)—Essays on the Conario-Hypophysial Tract, &c. 8vo. 1883.
Perry, Rev. S. J. F.R.S. (the Author)—Results of Meteorological and Magnetical Observations, Stonyhurst, 1882. 12mo. 1883.
Pharmaceutical Society of Great Britain—Journal, July–Oct. 1883. 8vo.
Photographic Society—Journal, New Series, Vol. VII. No. 9; Vol. VIII. No. 1. 8vo. 1883.
Physical Society of London—Proceedings, Vol. V. Part 4. 8vo. 1883.
Preussische Akademie der Wissenschaften—Sitzungsberichte, XXII.–XXXVII. 4to. 1883.
Rennie, James, Esq. F.R.S. M.R.I.—Samuel Sharpe. By P. W. Clayden. 12mo. 1883.
Hothouse Education. By J. A. Digby. 8vo. 1882.

Rio de Janeiro, Observatoire Imperiale—Bulletin, Nos. 5, 6, 7. fol. 1883.
Royal College of Surgeons of England—Calendar. 8vo. 1883.
Royal Irish Academy—Transactions, Vol. XXVIII. Parts 4–13. 4to. 1881–3.
Proceedings, Series II. Vol. II. Parts 3, 4; Vol. III. Parts 7–10. 8vo. 1881–3.
Royal Society of London—Proceedings. Nos. 225, 226. 8vo. 1883.
Siemens, Sir William, D.C.L. LL.D. F.R.S. M.R.I. (the Author)—On the Dependence of Radiation on Temperature. (Proc. Roy. Soc.) 8vo. 1883.
Reply to Mr. E. H. Cook on Conservation of Solar Energy. (Phil. Mag.) 8vo. 1883.
St. Pétersbourg Académie des Sciences—Mémoires, Tome XXXI. Nos. 1–4. 4to. 1883.
Smithsonian Institution, Washington—Smithsonian Miscellaneous Collections, Vols. XXII. to XXVII. 8vo. 1882–3.
Annual Report for 1881. 8vo. 1883.
Society for Psychical Research—Proceedings, Vol. I. Parts 2 and 3. 8vo. 1883.
Society of Arts—Journal, July–Oct. 8vo. 1883.
Spratt, Vice-Admiral T.A.B. C.B. F.R.S. &c. (the Author)—Report on Navigation of River Mersey (1882). 8vo. 1883.
Statistical Society—Journal, Vol. XLVI. Parts 2, 3. 8vo. 1883.
Telegraph Engineers, Society of—Journal, Vol. XII. No. 49. 8vo. 1883.
Tokio University—Memoirs, No. 5, Appendix. 4to. 1882.
United Service Institution, Royal—Journal, No. 120. 8vo. 1883.
Upsal University—Bulletin Mensuel de l'Observatoire Meteorologique, Vol. XIV. 4to. 1882–3.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1883: Heft 6, 7. 4to.
Victoria Institute—Journal, No. 66. 8vo. 1883.
Wisconsin Academy—Transactions, Vol. V. 8vo. 1882.
Zoological Society—Transactions, Vol. XI. Parts 8, 9. 4to. 1883.
Proceedings, 1882, Part 4; 1883, Parts 1–3. 8vo.
List of the Animals. 8vo. 1883.

GENERAL MONTHLY MEETING,

Monday, December 3, 1883.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Henry Brown, Esq. B.A. Oxon,
James Duncan, Esq. F.C.S.
William Thomas Sugg, Esq. C.E.
Mrs. Mary Willis Tanner,
Thomas Tyrer, Esq. F.C.S.

were elected Members of the Royal Institution.

Seven Candidates for Membership were proposed for election.

The decease of Sir William Siemens, Manager and Vice-President, on November 19th, was announced from the Chair.

The following Resolution, passed by the Managers at their Meeting this day, was read:—

"In the death of Sir WILLIAM SIEMENS the Royal Institution has lost an eminent member and a generous friend. The outcome of his great practical researches was frequently brought before us in lectures delivered in our theatre; he was our benefactor in presenting to us apparatus of great value; while his wise counsel, as a Manager, was ever ready when the interests of the Institution required it. He showed his veneration for one of its Professors by naming a vessel—a model one of its kind—constructed under his personal supervision for the transport and laying down of telegraphic cables, 'The Faraday.' In everything he touched, practical genius, guided by a knowledge of principles not frequent among practical men, was displayed. In the domains of heat, electricity, and metallurgy he won his chief renown; and here the ultimate issues of his labours are at present incalculable. England, the land of his adoption, has lost through his death an engineer of singular power, penetration, and many-sidedness. The source of the quality last mentioned, by which he was characterised, was, first of all, inherent ability, and, secondly, the comprehensive scientific education which he received in the seminaries and universities of his native land. He came to us thoroughly equipped with the theoretic knowledge necessary for practical ends, and he applied that knowledge successfully in the most varied spheres of action. As regards invention, he came of a family to the manner born: all his brothers, and especially his eldest brother, the celebrated Dr. Werner Siemens, having achieved distinction in applying science to the uses of life. William Siemens was a man of the most charming disposition, genial, kindly, without jealousy or bitterness, and as a natural result he secured not only the respect but the warm affection of those who intimately knew him. The Members who were present on the occasion of our last Monthly Meeting will not readily forget the animated description he then gave us from the Chair, of a new application of steam power which he had just seen tried on the River Spree, near Berlin. How little could the freshness and the vigour of that exposition prepare his hearers for the catastrophe so soon to follow! Among the Members of the Royal Institution he has left many mourning friends, who profoundly sympathise with his family in their great bereavement, and more especially with Lady Siemens in her irreparable loss."

The following Lecture Arrangements were announced:—

PROFESSOR DEWAR, M.A. F.R.S. *M.R.I.*—Six Experimental Lectures (adapted to a Juvenile Auditory) on ALCHEMY (in relation to Modern Science); on Dec. 27 (Thursday), Dec. 29, 1883; Jan. 1, 3, 5, 8, 1884.

REGINALD STUART POOLE, Esq. Keeper of Coins, British Museum.—Two Lectures on THE INTEREST AND USEFULNESS OF THE STUDY OF COINS AND MEDALS; on Tuesdays, Jan. 15, 22.

ERNST PAUER, Esq. Principal Professor of the Pianoforte at the Royal College of Music.—Six Lectures on THE HISTORY AND DEVELOPMENT OF THE MUSIC FOR THE PIANOFORTE AND ITS PREDECESSORS THE CLAVECIN, HARPSICHORD, &c. (with Musical Illustrations on these Instruments); on Thursdays, Jan. 17 to Feb. 21.

PROFESSOR TYNDALL, D.C.L. F.R.S. *M.R.I.*—Six Lectures on THE OLDER ELECTRICITY: ITS PHENOMENA AND INVESTIGATORS; on Thursdays, Feb. 28 to April 3.

ARCHIBALD GEIKIE, Esq. F.R.S. Director-General of the Geological Survey of the United Kingdom.—Five Lectures on THE ORIGIN OF THE SCENERY OF THE BRITISH ISLES; on Tuesdays, Jan. 29 to Feb. 26.

PROFESSOR JOHN G. MCKENDRICK, M.D. F.R.S.E. Prof. Inst. of Med. Univ. of Glasgow, Fullerian Prof. of Physiology, R.I.—Five Lectures on ANIMAL HEAT: ITS ORIGIN, DISTRIBUTION, AND REGULATION; on Tuesdays, March 4 to April 1.

Professor Henry Morley—Six Lectures on Life and Literature under Charles I.; on Saturdays, Jan. 19 to Feb. 23.

Captain W. de W. Abney, R.E. F.R.S. *M.R.I.*—Six Lectures on Photographic Action, considered as the Work of Radiation; on Saturdays, March 1 to April 5.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

from

The Governor-General of India—Geological Survey of India: Palæontologia Indica: Series X. Vol. II. Part 4; Series XII. Vol. IV. Part 1; Series XIII. Vol. I. Part 4, Fasc. 1 and 2. 4to. 1882–3.
Memoirs. Vol. XIX. Parts 2, 3, 4. 8vo. 1883.
The Lords of the Admiralty—Nautical Almanac for 1887. 8vo. 1883.
The Meteorological Office—Quarterly Weather Report, 1876, Part 2. fol. 1883.
Meteorological Atlas of the British Isles. fol. 1883.
Agricultural Society of England, Royal—Journal, Second Series, Vol. XIX. Part 2. 8vo. 1883.
Asiatic Society of Bengal—Journal, Vol. LII. Part 2, No. 1. 8vo. 1883.
Astronomical Society, Royal—Monthly Notices, Vol. XLIII. No. 9 Sup. 8vo. 1883.
Bankers, Institute of—Journal, Vol. IV. Part 9. 8vo. 1883.
Board of Trade—Report on Weights and Measures. fol. 1883.
British Architects, Royal Institute of—Proceedings, 1883–4, Nos. 2, 3. 4to.
Chemical Society—Journal for Nov. 1883. 8vo.
De La Rue, Warren, Esq. M.A. D.C.L. F.R.S. M.R.I. and Hugo W. Müller, Esq F.R.S. M.R.I. (the Authors)—Experimental Researches on the Electric Discharge with the Chloride of Silver Battery, Part IV. 4to. 1883.
East India Association—Journal, Vol. XV. No. 6. 8vo. 1883.
Editors—American Journal of Science for Nov. 1883. 8vo.
Analyst for Nov. 1883. 8vo.
Athenæum for Nov. 1883. 4to.
Chemical News for Nov. 1883. 4to.
Engineer for Nov. 1883. fol.
Horological Journal for Nov. 1883. 8vo.
Iron for Nov. 1883. 4to.
Nature for Nov. 1883. 4to.
Revue Scientifique and Revue Politique et Littéraire for Nov. 1883. 4to.
Steamship for Nov. 1883. fol.
Telegraphic Journal for Nov. 1883. 8vo.
Franklin Institute—Journal, No. 695. 8vo. 1883.
Geological Society—Quarterly Journal, No. 156. 8vo. 1883.
Glasgow Philosophical Society—Proceedings, Vol. XIV. 8vo. 1883.
Jenkins, Rev. Robert C. M.A. M.R.I. (the Author)—The Parents and Kinsfolk of Luther. (O 17) 16mo. 1883.
Manchester Geological Society—Transactions, Vol. XVII. Part 10. 8vo. 1883.
Medical and Chirurgical Society—Transactions, Vol. LXVI. 8vo. 1883.
Melbourne University, The Council of the—Calendar for 1882–3. 16mo. 1883.
Pharmaceutical Society of Great Britain—Journal, Nov. 1883. 8vo.
Reynolds, Messrs. F. W. and Co.—Illustrated Catalogue of Wood Working Machinery, &c. 4to. 1882.
Rio de Janeiro, Observatoire Impériale—Bulletin, No. 8. fol. 1883.
Society of Arts—Journal, Nov. 8vo. 1883.
Tidy, Charles Meymott, Esq. M.B. F.C.S. M.R.I. (the Author)—Legal Medicine, Part 2. 8vo. 1883.
United Service Institution, Royal—Journal, No. 121. 8vo. 1883.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1883: Heft 8. 4to.

Professor Tyndall, D.C.L. LL.D. F.R.S. *M.R.I.*

On Count Rumford, Originator of the Royal Institution.

(Lectures delivered May 3, 10, and 17, 1883.)

On a bright calm day in the autumn of 1872—that portion of the year called, I believe, in America the Indian summer—I made a pilgrimage to the modest birthplace of Count Rumford, the originator of the Royal Institution. My guide on the occasion was Dr. George Ellis of Boston, and a more competent guide I could not possibly have had. To Dr. Ellis the American Academy of Arts and Sciences had committed the task of writing a life of Rumford, and this labour of love had been accomplished in 1871, a year prior to my visit to the United States. In regard to Rumford's personal life, Dr. Ellis's elaborate volume constitutes, if I may so speak, the quarry out of which the building materials of these lectures are drawn. The life of such a man, however, cannot be duly taken in without reference to his work, and the publication by the American Academy of Sciences of four large volumes of Rumford's essays renders the task of dealing with his labours lighter than it would have been, had his writings been suffered to remain scattered in the magazines, journals, and transactions of learned societies to which they were originally communicated.

The name of Count Rumford was Benjamin Thompson. For thirty years he was the contemporary of another Benjamin, who reached a level of fame as high as his own. Benjamin Franklin and Benjamin Thompson were born within twelve miles of each other, and for six of the thirty years just referred to the one lived in England and the other in France. Yet there is nothing to show that they ever saw each other, or were in any way acquainted with each other, or, indeed, felt the least interest in each other. As regards posthumous fame, Rumford has in England fared worse than Franklin. For ten, or perhaps a hundred, people in this country, who know something of the career of the one, hardly a unit is to be found acquainted with the career of the other. Among scientific men, however, the figure of Rumford presents itself with singular impressiveness at the present day—a result mainly due to the establishment of the grand scientific generalisation known as the Mechanical Theory of Heat. Boyle, and Hooke, and Locke, and Leibnitz, had more or less distinctly ranged themselves on the side of this theory. But by experiments conducted on a scale unexampled at the time, and by reasonings, founded on these experiments, of singular force and penetration, Rumford has made himself a conspicuous landmark

in the history of the theory. His inference from his experiments was, as many of you know, that heat is a form of motion.

The town of Woburn, connected in my memory with a cultivated companion, with genial sunshine and the bright colouring of American trees, is nine miles distant from the city of Boston. In North Woburn, a little way off, on March 26th, 1753, Rumford was born. He came of people who had to labour for their livelihood, who tilled their own fields, cut their own timber and fuel, worked at their varied trades, and thus maintained the independence of New England yeomen. Thompson's father died when he was two years old. His mother married again, and had children by her second husband; but the affection between her and her first-born remained strong and unbroken to the end of her life. The arrangements made for the maintenance of mother and son throw some light upon their position. She was to have the use of one-half of a garden; the privilege of land to raise beans for sauce; to receive within a specified time 80 weight of beef, 8 bushels of rye, 2 bushels of malt, and 2 barrels of cider. Finally, she had the liberty of gathering apples to bake, and three bushels of apples every year.

The fatherless boy had been placed under the care of a guardian, from whom his stepfather, Josiah Pierce, received a weekly allowance of two shillings and fivepence for the child's maintenance. Young Thompson received his first education from Mr. John Fowle, a graduate of Harvard College, described by Dr. Ellis as "an accomplished and faithful man." He also went to a school at Byfield, kept by a relation of his own. At the age of eleven, he was placed for a time under the tuition of Mr. Hill, "an able teacher in Medford," adjoining Woburn. The lad's mind was ever active, and his invention incessantly exercised, but for the most part on subjects beside his daily work. In relation to that work, he came to be regarded as "indolent, flighty, and unpromising." His guardians, at length thinking it advisable to change his vocation, apprenticed him in October 1766, to Mr. John Appleton, of Salem, an importer of British goods. Here, however, instead of wooing customers to his master's counter, he occupied himself with tools and implements hidden beneath it. He is reported to have been a skilful musician, passionately fond of music of every kind; and during his stay with Mr. Appleton, whenever he could do so without being heard, he solaced his leisure by performances on the violin.

By the Rev. Thomas Barnard, minister of Salem, and his son, young Thompson was taught algebra, geometry, and astronomy. By self-practice, he became an able and accurate draughtsman. He did not escape that last infirmity of ingenious minds—the desire to construct a perpetual motion. He experimented with fireworks, and was once seriously burnt by the unexpected ignition of his materials. His inquisitiveness is illustrated by the questions put to his friend Mr. Baldwin in 1769. He wishes to be told the direction pursued by the

rays of light under certain conditions; he desires to know the cause of the change of colour which fire produces in clay. "Please," he adds, "to give the nature, essence, beginning of existence, and rise of the wind in general, with the whole theory thereof, so as to be able to answer all questions relative thereto." One might suppose him to be preparing for a competitive examination. He grew expert in drawing caricatures, a spirited group of which has been reproduced by Dr. Ellis. It is called a Council of State, and embraces a jackass with twelve human heads. These sketches were found in a mutilated scrap-book, which also contains a kind of journal of his proceedings in 1769, when he changed his place in Salem for a situation in a dry-goods store in Boston. He mentions a French class which he attended in the evenings, records the purchase of a certain measure of black cloth, states his debt to his uncle Hiram Thompson for part of the rent of a pew. The liabilities thus incurred he met by cutting and carting firewood. Mixed with entries such as these are "directions for the backsword," in which the postures of the combatants are defined, and illustrated by sketches. The scrap-book also contains an account of the expense he had been at "towards getting an electrical machine." Soon afterwards he began the study of medicine under Dr. John Hay, of Woburn.

Thompson keeps a strict account of his debts to Dr. Hay; credits him with a pair of leather gloves; credits Mrs. Hay with knitting him a pair of stockings. These items he tacks on to the more serious cost of his board from December 1770 to June 1772 at forty shillings, old currency, per week, amounting to 156*l.* The specie payments of Thompson were infinitesimal, eight of them amounting in the aggregate to 2*l.* His further forms of payment illustrate the habits of the community in which he dwelt. Want of money caused them to fall back upon barter. He debits Dr. Hay with the following items, the value of which no doubt had been previously agreed upon between them. "To ivory for smoke machine; parcels of butter, coffee, sugar, and tea, parcels of various drugs, camphor, gum benzoine, arsenic, calomel, and rhubarb: one-half of white sheep-skin leather; brass wire; white oak timber; to sundry lots of wood; to other lots delivered while I was at Wilmington, and left by me when I was at Wilmington the last time; to a blue Huzza cloak, bought of Zebediah Wyman, and paid for by fifteen and a half cords of wood; a pair of knee buckles; a chirurgical knife; to a cittern, and to the time I have been absent from your house, nineteen weeks at forty shillings; and for the time my mother washed for me." To help him, moreover, to eke out the funds necessary for the prosecution of his studies, Thompson tried his hand from time to time at school teaching.

At this early age, for he was not more than seventeen, he had learnt not only the importance of order in the distribution of his working time, but also the importance of exercise and relaxation. The four and twenty hours of a single day are thus spaced out. "From eleven to six, sleep. Get up at six o'clock and wash my

hands and face. From six to eight, exercise one half, and study one half. From eight to ten, breakfast, attend prayers, &c. From ten to twelve, study all the time. From twelve to one, dine, &c. From one to four, study constantly. From four to five, relieve my mind by some diversion or exercise. From five till bedtime, follow what my inclination leads me to; whether it be to go abroad, or stay at home and read either Anatomy, Physic, or Chemistry, or any other book I want to peruse."

In 1771 he managed, by walking daily from Woburn to Cambridge and back, a distance of some sixteen miles, to attend the lectures on natural philosophy delivered by Professor Winthrop in Harvard College. This privilege was secured to him by his friend Mr. Baldwin, with whom about this time he appears to have quarrelled. The difference, however, was rapidly adjusted, and it left no abiding trace behind. Thompson had taught school for a short time at Wilmington, and afterwards for six weeks and three days at Bradford, where his repute rose so high that he received a call to Concord, the capital town of New Hampshire, situated higher up than Bradford on the river Merrimac. The Indian name of Concord was Penacook. In 1733 it had been incorporated as a town in Essex county, Massachusetts. Some of the early settlers in Essex county had come from the English Essex; and, as regards pronunciation, they carried with them the name of the English Essex town, Romford, of brewery celebrity. They, however, changed the first *o* into *u*, calling the American town Rumford. Strife had occurred as to the county or state to which Rumford belonged. But the matter was amicably settled at last; and to denote the subsequent harmony, the name was changed from Rumford to Concord.* In later years, when honours

---

* In connection with this subject I have been favoured with the following interesting letter:—

"ADDISON LODGE, BARNES, S.W.

"DEAR SIR, *August 19th.*

"I venture to proffer a remark upon a detail in your interesting paper upon *Count Rumford.* My apology for so doing is that I am a Romford man, and that I think you may care for the mere crumb of information I possess bearing upon the spelling and pronunciation of the name of my native place.

"Romford is always pronounced Rumford by Essex folk. When I was a boy it was *spelled* almost indifferently, Romford and Rumford. I remember that the post-mark in my school days (some forty years ago) was Rumford. Norden's map of Essex (1599) has Rumforde; and on Bowen's map (1775) the spelling is the same—Rumford. The registers in the vestry book, from 1665 until some fifty years ago, give *Rumford.* So that I think it safe to say that the traditional spelling and pronunciation with the Essex settlers at Concord must have been Rumford. I must, however, add—but I fear I am hardly justified in troubling you with so long a note—that the *o* occurs in two *Latin* entries in the Register—

"'1564, Baptizata fuit Anna Baylie filia Hugonis Cissor, Romford.'

"And in the same year there is an entry of a burial with 'Romfordiae.' I believe it was the *Latinizing* of Rumford that modified the vowel, the alteration being prompted by the mistaken notion that the etymology of the place was

fell thick upon him, Rumford was made a Count of the Holy Roman Empire. He chose for his title Count Rumford, in memory of his early association with Concord.

"When Benjamin Thompson went to Concord as a teacher, he was in the glory of his youth, not having yet reached manhood. His friend Baldwin describes him as of a fine manly make and figure, nearly six feet in height, of handsome features, bright blue eyes, and dark auburn hair. He had the manners and polish of a gentleman, with fascinating ways, and an ability to make himself agreeable."* In Concord, at the time of Thompson's arrival, there dwelt the widow of a Colonel Rolfe with her infant son. Her husband had died in December 1771, leaving a large estate behind him. Rumford was indebted to Mrs. Rolfe's father, the Rev. Timothy Walker, minister of Concord, for counsel, and to her brother for civility and hospitality. There the widow and the teacher met, and their meeting was a prelude to their marriage. Rumford, somewhat ungallantly, told his friend Pictet in after years that she married him rather than he her. She was obviously a woman of decision. As soon as they were engaged, an old curricle, left by her father, was fished up, and, therein mounted, she carried Thompson to Boston, and committed him to the care of the tailor and hairdresser. This journey involved a drive of sixty miles. On the return, it is said, they called at the house of Thompson's mother, who, when she saw him, exclaimed, "Why, Ben, my son, how could you go and lay out all your winter's earnings in finery?" Thompson was nineteen when he married, his wife being thirty-three.

In 1772 he became acquainted with Governor Wentworth, then resident at Portsmouth. On the 13th of November there was a grand military review at Dover, New Hampshire, ten miles from Portsmouth, at which Thompson was present. On two critical occasions in the life of this extraordinary man his appearance on horseback apparently determined the issues of that life. As he rode among the soldiers at Dover, his figure attracted the attention of the governor, and on the day following, he was the great man's guest. So impressed was Wentworth with his conversation that he at once made up his mind to attach Thompson to the public service. To secure this

---

Roman-ford. That the Rum is English (= broad) is, I think, hardly open to question. The nearest *ford* town is *Il*ford, with which the *roomy* ford contrasts. Of late the sluggish little river has come to be called the river Rom. This is quite a novel 'notion' and is quite local.

"Thanking you for the pleasure and profit I have derived from reading your article,

"I remain, dear Sir,

"Yours very faithfully,

"HENRY ATTWELL.

"Professor Tyndall, F.R.S.
&c. &c. &c."

* Ellis, p. 43.

wise end he adopted unwise means. "A vacancy having occurred in a majorship in the Second Provincial Regiment of New Hampshire, Governor Wentworth at once commissioned Thompson to fill it." Jealousy and enmity naturally followed the appointment of a man without name or fame in the army, over the heads of veterans with infinitely stronger claims. He rapidly, however, became a favourite with the governor, and on his proposing, soon after his appointment, to make a survey of the White Mountains, Wentworth not only fell in with the idea, but promised, if his public duties permitted, to take part in the survey himself. It will be remembered that at this time Thompson was not quite twenty years old.

For a moment, in 1773, he appears in the character of a farmer, and invokes the aid of a friend to procure for him supplies of grass and garden seeds from England. But amid pre-occupations of this kind his scientific bias emerges. After a brief reference to the seed procured for him by his friend Baldwin, he proposes to the latter the following question: "A certain cistern has three brass cocks, one of which will empty it in 15 minutes, one in 30 minutes, and the other in 60 minutes. Qu.—How long would it take to empty the cistern if all three cocks were to be opened at once? If you are fond of a correspondence of this kind, and will favour me with an easy question, Arithmetical or Algebraical, I will endeavour to give as good an account of it as possible. If you find out an answer to the above immediately, I hope you will not take it as an affront, my proposing anything which you may think so easy, for I must confess I scarce ever met with any little notion that puzzled me so much in my life."

In 1774 the ferment of discontent with the legislation of the mother country had spread throughout the colony. Clubs and committees were formed which often compelled men to take sides before the requisite data for forming a clear judgment had been obtained. "Our candour," says Dr. Ellis, "must persuade us to allow that there were reasons, or at least prejudices and apprehensions which might lead honest and right-hearted men, lovers and friends of their birthland, to oppose the rising spirit of independence as inflamed by demagogues, and as foreboding discomfiture and mischief." Thompson became "suspect," though no record of any unfriendly or unpatriotic act or speech on his part is to be found. He was known to be on friendly terms with Governor Wentworth, but the governor, when he gave Thompson his commission, was highly popular in the province. Prior to Wentworth's accession to office he "had strongly opposed every measure of Great Britain which was regarded as encroaching upon our liberties." He thought himself, nevertheless, in duty bound to stand by the royal authority when it was openly defied; and this naturally rendered him obnoxious.

Thompson was a man of refractory temper, and the circumstances of the time were only too well calculated to bring that temper out.

"There was something," says Dr. Ellis, "exceedingly humiliating and degrading to a man of an independent and self-respecting spirit in the conditions imposed at times by the 'Sons of Liberty,' in the process of cleansing oneself from the taint of Toryism. The Committees of Correspondence and of Safety, whose services stand glorified to us through their most efficient agency in a successful struggle, delegated their authority to every witness or agent who might be a self-constituted guardian of patriotic interests, or a spy, or an eavesdropper, to catch reports of suspected persons." Human nature is everywhere the same, and to protect a cherished cause these "sons of liberty" sometimes adopted the tactics of the papal inquisition. On the 8th of November, 1774, for example, one Nicholas Austin had to go down upon his knees and make the following confession: "Before this company I confess I have been aiding and assisting in sending men to Boston to build barracks for the soldiers to live in, at which you have justly reason to be offended, which I am sorry for, and humbly ask your forgiveness; and I do affirm, that for the future I never will be acting or assisting in any wise whatever, in act or deed, contrary to the constitution of the country; as witness my hand."

Public feeling grew day by day more exasperated against Thompson, and in the summer of 1774, he was summoned before a committee to answer to the charge of being unfriendly to the cause of liberty. "He denied the charge, and challenged proof. The evidence, if any such was offered—and no trace of testimony, or even of imputation of that kind is on record—was not of a sort to warrant any proceeding against him, and he was discharged." The discharge, however, gave him but little relief, and extra-judicial plots were formed against him. The Concord mob resolved to take the matter into their own hands. One day they collected round his house, and with hoots and yells demanded that Thompson should be delivered up to them. Having got wind of the matter, he escaped in time; and on the assurance of Mrs. Thompson and her brother Colonel Walker that he had quitted Concord, the mob dispersed.

In a letter addressed to his father-in-law at this time from Charlestown near Boston, he gives his reasons for quitting home. "To have tarried at Concord and have stood another trial at the bar of the populace would doubtless have been attended with unhappy consequences, as my innocence would have stood me in no stead against the prejudices of an enraged infatuated multitude—and much less against the determined villany of my inveterate enemies, who strive to raise their popularity on the ruins of my character."

He returned to his mother's house in Woburn, where he was joined by his wife and child. While they were with him, shots were exchanged and blood was shed at Concord and Lexington. Thompson was at length arrested, and confined in Woburn. A "Committee of Correspondence" was formed to inquire into his conduct. They invited every one who could give evidence in the affair to appear at

the meeting house on the 18th of May. The committee met, but finding nothing against the accused, they adjourned the meeting. He then addressed a petition to the Committee of Safety for the colony of Massachusetts Bay, in which he begged for a full and searching trial, relying on an acquittal commensurate with the thoroughness of the examination. The petition was not attended to. On the 29th of May, 1775, he was examined at Woburn, where he conducted his own defence. He was acquitted by the committee, who recommended him to the "protection of all good people in this and the neighbouring provinces." The committee, however, refused to make this acquittal a public one, lest, it was alleged, it should offend those who were opposed to Thompson.

Despair and disgust took possession of him more and more. In a long letter addressed to his father-in-law from Woburn, he defends his entire course of conduct. His principal offence was probably negative; for silence at the time was deemed tantamount to antagonism. During his brief period of farming, he had working for him some deserters from the British army in Boston. These he persuaded to go back, and this was urged as a crime against him. He defended himself with spirit, declaring, after he had explained his motives, that if his action were a crime, he gloried in being a criminal. He made up his mind to quit the country, expressing the devout wish "that the happy time may soon come when I may return to my family in peace and safety, and when every individual in America may sit down under his own vine and under his own fig-tree, and have none to make him afraid."

On this letter, and on the circumstances of the time, Dr. Ellis makes the following pertinent remarks: "Major Thompson was not the only person in those troubled times that had occasion to charge upon those espousing the championship of public liberty a tyrannical treatment of individuals who did not accord with their schemes or views. Probably in our late war of rebellion his case was paralleled by those of hundreds in both sections of our country, who with halting and divided minds or unsatisfied judgments, were arrested in the process of decision by treatment from others which put them under the lead of passion. The choice of a great many Royalists in our revolution would have been wiser and more satisfactory to themselves, had they been allowed to make it deliberately." On October 13th, 1775, Thompson quitted Woburn, reached the shore of Narragansett Bay and went on board a British frigate. In this vessel he was conveyed to Boston, where he remained until the town was evacuated by the British troops. The news of this catastrophe he carried to England. Henceforward, till the close of the war, he was on the English side. As a matter of course he was proscribed by his countrymen, and his property confiscated.

Thompson was not only a man of great capacity, but, in early days, of a social pliancy and teachableness which enabled him with extreme rapidity to learn the manners and fall into the ways of great people.

On the English side the War of Independence was begun, continued, and ended, in ignorance. Blunder followed blunder, and defeat followed defeat, until the knowledge which ought to have been ready at the outset came too late. Thompson for a time was the vehicle of such belated knowledge. He was immediately attached to the Colonial Office, then ruled over by Lord George Germain. Cuvier, in his 'Eloge,' thus describes his first interview with that minister: "On this occasion by the clearness of his details and the gracefulness of his manners, he insinuated himself so far into the graces of Lord George Germain that he took him into his employment." With Lord George he frequently breakfasted, dined, and supped, and was occasionally his guest in the country. But besides giving information useful to his chiefs, he occupied himself with other matters. He was a born experimentalist, handy, ingenious, full of devices to meet practical needs. He turned his attention to improvements in military matters; "advised and procured the adoption of bayonets for the fusees of the Horse Guards, to be used in fighting on foot." He had previously been engaged with experiments on gunpowder, which he now resumed. The results of these experiments he communicated to Sir Joseph Banks, then President of the Royal Society, with whom he soon became intimate. In 1779, he was elected Fellow of the Royal Society.

When the war had become hopeless, many of the exiles who had been true to the Royalist cause came to England, where Thompson's official position imposed on him the duty of assuaging their miseries and adjusting their claims. In this connection, the testimony of Dr. Ellis regarding him is that "so far as the relations between these refugees and Mr. Thompson can be traced, I find no evidence that he failed to do in any case what duty and friendliness required of him." Still he did not entirely escape the censure of his outlawed fellow-countrymen. One of them in particular had been a judge in Salem when Thompson was a shop-boy in Appleton's store. Judge Curwen complained of Thompson's fair appearance and uncandid behaviour. He must have keenly felt the singular reversal in their relations. "This young man," says the judge, "when a shop-lad to my next neighbour, ever appeared active, good-natured, and sensible; by a strange concurrence of events, he is now Under-Secretary to the American Secretary of State, Lord George Germain, a Secretary to Georgia, Inspector of all the clothing sent to America, and Lieutenant-Colonel Commandant of Horse Dragoons at New York; his income from these sources is, I have been told, near 7000*l.** a year—a sum infinitely beyond his most sanguine expectations."

As the prospects of the war darkened, Thompson's patron in England became more and more the object of attack. The people had been taxed in vain. England was entangled in Continental war, and it became gradually recognised that the subjugation of the colony

* This Dr. Ellis considers to be a delusion.

was impossible. Burgoyne had surrendered, and the issue of the war hung upon the fate of Cornwallis. On October 19th he was obliged to capitulate. The effect of the disaster upon Lord North, who was then Prime Minister, is thus described by Sir M. W. Wraxall:—"The First Minister's firmness, and even his presence of mind, gave way for a short time under this awful disaster. I asked Lord George afterwards how he took the communication. 'As he would have taken a ball in his breast,' replied Lord George. 'For he opened his arms, exclaiming wildly, as he paced up and down the apartment during a few minutes, O God! it is all over!'"

To Thompson's credit be it recorded, that he showed no tendency to desert the cause he had espoused when he found it to be a failing one. In 1782 his chief was driven from power, and at this critical time he accepted the commission of lieutenant-colonel in the British army, and returned to America with a view of rallying for a final stand such forces as he might find capable of organisation. He took with him four pieces of artillery, with which he made experiments upon the voyage. His destination was Long Island, New York, but stress of weather carried him to Charleston, South Carolina. Here the influence of his presence and energy was soon felt. "The regiment of cavalry," says Pictet, "called the King's American Dragoons was raised at this time in his native country by his friends and agents, and he was then commissioned as its lieutenant-colonel commandant. This circumstance led him to return to America to serve with his regiment. He had intended to land at New York, but contrary winds compelled him to disembark at Charleston. Obliged to pass the winter there, he was made commander of the remains of the cavalry in the royal army, which was then under the orders of Lieutenant-General Leslie. This corps was broken, but he promptly restored it, and won the confidence and attachment of the commander. He led them often against the enemy, and was always successful in his enterprises."

About the middle of April Thompson reached New York, and took command of the King's American Dragoons. Colours were presented to the regiment on August 1st, a very vivid account of the ceremony being given in Rivington's *Royal Gazette* of August 7th, 1782. Prince William Henry, afterwards King William the Fourth, was there at the time. The regiment passed in review before him, performing marching salutes. They then returned, dismounted, and formed in a semicircle in front of the canopy. After an address by their chaplain, the whole regiment knelt down, laid their helmets and arms on the ground, held up their right hands, and took a most solemn oath of allegiance to their sovereign, and fidelity to their standard. From Admiral Digby the prince received the colours, and presented them with his own hands to Thompson, who passed them on to the oldest cornets. "On a given signal the whole regiment, with all the numerous spectators, gave three shouts, the music played

'God save the King,' the artillery fired a royal salute, and the ceremony was ended." *

Many complaints have been made of the behaviour of the troops during their stay at Long Island, New York. But war is always horrible; and it is pretty clear, from the account of Dr. Ellis, that the complaints had no other foundation than events inseparable from the carrying on of war. In the statement of Thompson's case, his biographer, extenuating nothing, and setting down naught in malice, winds up his third chapter with these words: "Having thus pronounced upon him as in opposition, in act, to himself and his convictions, I may add to such praise as is due to him as a good soldier, quick and true and bold in action, and faithful to the government which he served, the higher tribute that from the hour when the war closed, he became, and ever continued to be, the constant friend and generous benefactor of his native country."

Early in April 1783, he obtained leave to return to England, but, finding there no opportunity for active service, he resolved to try his fortune on the Continent, intending to offer his services as a volunteer in the Austrian army against the Turks. The historian Gibbon crossed the Channel with him. In a letter dated Dover, September 17th, 1783, Gibbon writes thus:—"Last night, the wind was so high that the vessel could not stir from the harbour; this day it is brisk and fair. We are flattered with the hope of making Calais Harbour by the same tide in three hours and a half; but any delay will leave the disagreeable option of a tottering boat or a tossing night. What a cursed thing to live in an island! this step is more awkward than the whole journey. The triumvirate of this memorable embarkation will consist of the grand Gibbon, Henry Laurens, Esq., President of Congress; and Mr. Secretary, Colonel, Admiral, Philosopher Thompson, attended by three horses, who are not the most agreeable fellow-passengers. If we survive, I will finish and seal my letter at Calais. Our salvation shall be ascribed to the prayers of my lady and aunt, for I do believe they both pray." The "grand Gibbon" is reported to have been terribly frightened by the plunging of his fellow-passengers, the three blood horses.

Thompson pushed on to Strasburg, where Prince Maximilian of Bavaria, then a field marshal in the service of France, was in garrison. As on a former occasion in his native country, Thompson, mounted on one of his chargers, appeared on the parade ground. He attracted the attention of the Prince, who spoke to him, and, on learning that he had been serving in the American war, pointed to some of his officers, and remarked that they had been in the same war. An animated conversation immediately began, at the end of which Thompson was

* Whether Thompson at this time made any effort to communicate with the members of his own family is not known. To do so would have been difficult, but not impossible.

invited to dine with the Prince. After dinner, it is said, he produced a portfolio containing plans of the principal engagements, and a collection of excellent maps of the seat of war. Eager for information, the Prince again invited him for the next day, and when at length the traveller took leave, engaged him to pass through Munich, giving him a friendly letter to his uncle, the Elector of Bavaria.

Thompson carried with him wherever he went the stamp of power and the gift of address. The Elector, a sage ruler, saw in him immediately a man capable of rendering the state good service. He pressed his visitor to accept a post half military and half civil. The proposal was a welcome one to Thompson, and he came to England to obtain the king's permission to accept it. Not only was the permission granted, but on February 23rd, 1784, he was knighted by the king. Dr. Ellis publishes the "grant of arms" to the new knight. In it he is described as "Son of Benjamin Thompson, late of the Province of Massachusetts Bay in New England, Gent., deceased, and as one of the most antient Families in North America; that an Island which belonged to his Ancestors, at the entrance of Boston Harbour, where the first New England Settlement was made, still bears his name; that his Ancestors have ever lived in reputable Situations in that country where he was born, and have hitherto used the Arms of the antient and respectable Family of Thompson, of the county of York, from a constant Tradition that they derived their Descent from that Source." The original parchment, perfect and unsullied, with all its seals, is in the possession of Mrs. James F. Baldwin, of Boston, widow of the executor of Countess Sarah Rumford.* The knight himself, observes his biographer, must have furnished the information written on that flowery parchment. Thompson was fond of display, and he here gave rein to his tendency. He returned to Munich, and on his arrival the Elector appointed him colonel of a regiment of cavalry and general aide-de-camp to himself. He was lodged in a palace, which he shared with the Russian Ambassador, and had a military staff and a corps of servants. "His imposing figure, his manly and handsome countenance, his dignity of bearing, and his courteous manners, not only to the great, but equally to his subordinates and inferiors, made him exceedingly popular."

He soon acquired a mastery of the German and French languages. He made himself minutely acquainted with everything concerning the dominions of the Elector—their population, and employments, their resources and means of development, and their relations to other powers. He found much that urgently needed removal and much that required reformation. Speaking of the Electorate, Cuvier remarks that "its sovereigns had encouraged devotion, and made no stipulation in favour of industry. There were more convents than manufactories in their states; their army was almost a shadow, while ignorance and idleness were conspicuous in every class of society."

* Ellis.

Thompson, however, evoked no religious animosity. He avowed himself a Protestant, but met with no opposition on that score. Holding as he did the united offices of Minister of War, Minister of Police, and Chamberlain of the Elector, his influence and action extended to all parts of the public service. Then, as now, the armies of the continent were maintained by conscription. Drawn away from their normal occupations, the rural population returned after their term of service lazy and demoralised. This was a great difficulty, in dealing with which patient caution had to combine with administrative skill. Four years of observation were spent at Munich, before Thompson attempted anything practical. The pay of the soldiers was miserable, their clothing bad, their quarters dirty and mean; the expense being out of all proportion to the return. The officers, as a general rule, regarded the soldiers as their slaves; and here special prudence was necessary in endeavouring to effect a change. The more earnest among the officers were induced to co-operate with him, by making the reforms which he sought to introduce to originate apparently with them. He aimed at making soldiers citizens and citizens soldiers. The situation of the soldier was to be rendered pleasant, his pay was to be increased, his clothing rendered comfortable and even elegant, while all liberty consistent with strict subordination was to be permitted him. Within, the barracks were to be neat and clean; and without, attractive. Reading, writing, and arithmetic were to be taught, not only to the soldiers and their children, but to the children of the neighbouring peasantry. The paper used in the school would, it was urged, be practically free of cost, as it would serve afterwards for cartridges.

The marshes near Mannheim were dreary bogs, useless for cultivation and ruinous to the health of the city. Thompson drained them, banked them in, and converted them into a garden for the use of the garrison. For the special purpose of introducing the culture of the potato, he extended the plan of military gardens to all other garrisons. They were tilled, and their produce was owned, by non-commissioned officers and privates, each of whom had a plot of 365 square feet allotted to him. Gravel walks divided the plots from each other. The plan proved completely successful. Indolent soldiers became industrious, while the soldiers on furlough spreading abroad their taste and knowledge, caused little gardens to spring up everywhere over the country. Having secured this end, he converted it into a means of suppressing the enormous evils of mendicity. Bavaria was infested with beggars, vagabonds, and thieves, native and foreign. These mendicant tramps were in the main stout, healthy, and able-bodied fellows, who found a life of thievish indolence pleasanter than a life of honest work. "These detestable vermin had recourse to the most diabolical arts, and the most horrid crimes in the prosecution of their infamous trade." They robbed, and maimed and exposed little children, so as to extract money from the tender-hearted. In the cities the beggars formed a distinct caste,

with professional rules to guide them. Their training was a training in robbery; the means they employed for extorting support being equivalent to direct plunder. Seeing no escape from the incubus, the public had come to bow to it as a necessity. The energy with which Thompson grappled with this evil may be inferred from the fact that out of a population of sixty thousand, two thousand six hundred beggars were seized in a single week.

Four regiments of cavalry were so cantoned that every village in Bavaria and the adjoining provinces had a patrol party of four or five mounted soldiers "daily coursing from one station to another." The troopers were under strict discipline, extreme care being taken to avoid collision with the civil authorities. This disposition of the cavalry was antecedent to seizing, as a beginning, all the beggars in the capital. Aged and infirm mendicants were carefully distinguished from the sturdy and able-bodied. Voluntary contributions were essential, but the inhabitants, though groaning under the load of mendicancy, had been so often disappointed in their efforts to get rid of it, that they now held back. Thompson resolved to give proof of success before asking for general aid. He interested persons of high rank in his scheme; organised a bureau to relieve the needy and employ the idle. The members of his committee were Presidents of the great offices of State, who worked without pay. The city was divided into sixteen districts, with a committee of charity for each, while a respected citizen assisted by a priest and a physician, serving gratuitously, looked after the worthy poor. He knew perfectly well that in the city many bequests consecrated to charity were being abused and wasted, but he cautiously abstained from meddling with them.

The problem before him might well have daunted a courageous man. It was neither more nor less than to convert people bred up in lazy and dissolute habits into thrifty workers. Precepts, he knew, were unavailing, so his aim was to establish habits. Reversing the maxim that people must be virtuous to be happy, he made his beggars happy as a step towards making them virtuous. He affirmed that he had learnt the importance of cleanliness through observing the habits of birds and beasts. Lawgivers and founders of religions never failed to recognise the influence of cleanliness on man's moral nature. "Virtue," he said, "never dwelt long with filth and nastiness, nor do I believe there ever was a person scrupulously attentive to cleanliness who was a consummate villain." He had to deal with wretches covered with filth and vermin, to cleanse them, to teach them, and to give them the pleasure and stimulus of earning money. He did not waste his means on fine buildings, but taking a deserted manufactory, he repaired it, enlarged it, adding to it kitchen, bakehouse, and workshops for mechanics. Halls were provided for the spinners of flax, cotton, and wool. Other halls were set up for weavers, clothiers, dyers, saddlers, wool-sorters, carders, combers, knitters, and seamstresses.

The next step was to get the edifice filled with suitable inmates.

New Year's Day was the beggars' holiday, and their patron and reformer chose that day to get hold of them. It was the 1st of January, 1790. In the prosecution of his despotic scheme all men seemed to fall under his lead. To relieve it of the odium which might accrue if it were effected wholly by the military, he associated with himself and his field officers the magistrates of Munich. They gave him willing sympathy and aid. On New Year's morning he and the chief magistrate walked out together. With extended hand a beggar immediately accosted them. Thompson, setting the example to his followers, laid his hand gently upon the shoulder of the vagabond, committed him to the charge of a sergeant, with orders to take him to the Townhall, "where he would be provided for in one way if he were really helpless, but in another way if he were not." Thompson encouraged his associates, and with such alacrity was the work accomplished, that at the end of that day not a single beggar remained at large. The name of every member of the motley crew was inserted in prepared lists, and they were sent off to their haunts with instructions to appear on the following day at the military workhouse, where they would inhabit comfortable warm rooms, enjoy a warm dinner daily, and be provided with remunerative work. In the suburbs the same measures were followed up successfully by patrols of soldiers and police.

With his iron resolution was associated, in those days, a plastic tact which enabled him to avoid jealousies and collisions that a man of more hectoring temper and less self-restraint would infallibly have incurred. To the schools for poor students, the Sisters of Charity, the hospital for lepers, and other institutions had been conceded the right of making periodic appeals from house to house; German apprentices had also been permitted to beg upon their travels; all of these had their claims adjusted. After he had swept his swarm of paupers into the quarters provided for them, Thompson's hardest work began. Here the inflexible order which had characterised him through life came as a natural force to his aid. "He encouraged a spirit of industry, pride, self-respect, and emulation, finding help even in trifling distinctions of apparel." His pauper workhouse was self-supporting, while its inmates were happy. For several years they made up all the clothing of the Bavarian troops, realising sometimes a profit of 10,000 florins a year. Thompson himself constructed and arranged a kitchen which provided daily a warm and nutritive dinner for a thousand or fifteen hundred persons; an incredibly small amount of fuel sufficing to cook a dinner of this magnitude. The military workhouse was also remunerative. Its profits for six years exceeded a hundred thousand dollars. The military workhouse at Mannheim was unfortunately set on fire and ruined during the siege of the city by Austrian troops.

Thompson had the art of making himself loved and honoured by the people whom he ruled in this arbitrary way. Some very striking illustrations of this are given in the 'Life and Essays.' He once, for example, broke down at Munich under his self-imposed labours. It

was thought that he was dying, and one day while in this condition, his attention was attracted by the confused noise of a passing multitude in the street. It was the poor of Munich who were going in procession to the church to put up public prayers for him. "Public prayers!" he exclaims, "for me, a private person, a stranger, a Protestant!" Four years afterwards, when he was dangerously ill at Naples, the people, of their own accord, set apart an hour each evening, after they had finished their labours in the military workhouse, to pray for his recovery.

Men find pleasure in exercising the powers they possess, and Rumford possessed, in its highest and strongest form, the power of organisation. The relief of the poor, which occupied his attention for years, was pursued by him as a scientific inquiry. He differentiates the people who have fair claims upon the State from those whose infirmity and incapacity render continuous assistance necessary, but who cannot be aided by compulsory taxation. In this case the promptings of humanity must be invoked. Persons of high rank ought here to take the lead, combining with those immediately below them to secure efficient supervision and relief. The expense thus incurred is small compared with that incidental to beggary and its concomitant thieving. Thompson's hope and confidence never forsook him. He faced unquailing, problems from which less daring spirits would have recoiled. He held undoubtingly that "arrangement, method, provision for the minutest details, subordination, co-operation, and a careful system of statistics, will facilitate and make effective any undertaking, however burdensome and comprehensive." Such a statement would surely have elicited a bravo from Carlyle. In him flexible wisdom formed an amalgam with despotic strength. With skill and resolution the objects of public benevolence must be made to contribute as far as possible to their own support. The homeliest details do not escape him. He commends well dressed vegetables as a cheap and wholesome form of nutrition. He descants upon the potato, he gives rules for the construction of soup-kitchens, and determines the nutritive value of different kinds of food. During his boyhood at Woburn he had learnt the use of Indian corn, and at Munich he strongly recommends the dumplings, bread, and hasty pudding that may be made from maize. Pure love of humanity would at first sight seem to have been the motive force of Thompson's action. Still, it has been affirmed by those who knew him that he did not really love his fellow men. His work had for him the fascination of a problem above the capacities of most men, but which he felt himself able to solve. It was said to be the work of his intellect, not of his heart. In reference to him, Cuvier quotes what Fontenelle said of Dodard, who turned his rigid observance of the fasts of the Church into a scientific experiment on the effects of abstinence, thereby taking the path which led at once to heaven and into the French Academy. I should hesitate before accepting this as a complete account of Rumford's motives.

In the north-easterly environs of Munich a wild and neglected

region of forest and marsh, which had formerly been the hunting ground of the Elector, was converted by Thompson into an "English garden." Pleasure grounds, parks, and fields were laid out, and surrounded by a drive six miles long. Walks, promenades, grottos, a Chinese pagoda, a racecourse, and other attractions were introduced; a lake was formed, and a mound raised; while a refreshment saloon, handsomely furnished, provided for the creature comforts of the visitors. Here, during Thompson's absence in England in 1795, and without his knowledge, a monument was raised to commemorate his beneficent achievements. "It stands within the garden, and is composed of Bavarian freestone and marble. It is quadrangular, its two opposite fronts being ornamented with basso-rilievos, and bearing inscriptions." The wanderer, on one side, is exhorted to halt, while thankfulness strengthens his enjoyment. "A creative hint from Carl Theodor, seized upon with spirit, feeling, and love by the friend of man, Rumford, has ennobled into what thou now seest around thee this once desert region." On the other side of the monument is a dedication to "Him who eradicated the most scandalous of public evils, Idleness and Mendicity; who gave the poor help, occupation, and morals, and to the youth of the Fatherland so many schools of culture. Go, wanderer! try to emulate him in thought and deed, and us in gratitude."

Rumford's health, as already indicated, had given way, and in 1793 he went to Italy to restore it. He was absent for sixteen months, and during his absence was seriously ill at Naples. Had he been less filled with his projects, it might have been better for his health. Had he known how to employ the sanative power of nature, he would have kept longer in working order his vigorous frame. But the mountains of Maggiore were to him less attractive than the streets of Verona, where he committed himself to the planning of soup-kitchens. He made similar plans for other cities, so that to call his absence a holiday would be a misnomer. He returned to Munich in August 1794, slowly recovering, but not able to resume the management of his various institutions. In September 1798, after an absence of eleven years, he returned to London. Dr. Ellis describes him as "the victim of an outrage" on his arrival, the meaning of which seems to be that his trunk containing his papers, which was carried behind his carriage, was appropriated by London thieves. "By this cruel robbery," he says, "I have been deprived of the fruits of the labours of my whole life. . . . It is the more painful to me, as it has clouded my mind with suspicions that can never be cleared up." What the suspicions were we do not know.

Soon afterwards he was invited by Lord Pelham, then Secretary for Ireland, to visit him in Dublin; he went, and during his two months' stay there, busied himself with improvements in warming, cooking, and ventilation, in the hospitals and workhouses of the city. He left behind him a number of models of useful mechanism. The Royal Irish Academy elected him a member. The grand jury of

Dublin presented him with an address; while the Viceroy and the Lord Mayor wrote to him officially to thank him for his services. Dr. Ellis has not been able to find these documents, but they were seen by Pictet, who describes them as "filled with the most flattering expressions of esteem and gratitude."

In Rumford's case the life of the mind appeared to have interfered with the life of the affections. When he quitted America, he left his wife and infant daughter behind him, and whether there were any communications afterwards between him and them is not known. In 1793, in a letter to his friend Baldwin, he expressed the desire to visit his native country. He also wished exceedingly to be personally acquainted with his daughter, who was then nineteen. His affection for his mother, which appears to have been very real, also appears in his letter. With reference to this projected visit, he asks, "Should I be kindly received? Are the remains of party spirit and political persecutions done away? Would it be necessary to ask leave of the State?" A year prior to the date of this letter, Rumford's wife had died, at the age of fifty-two. On the 29th of January, 1796, his daughter, who was familiarly called "Sally Thompson," sailed for London to see her father. She had a tedious passage, but soon after her arrival she writes to her friend Mrs. Baldwin, "All fatigue and anxiety are now at an end, since my dear father is well and loves me."

In a 'History of her Life,' written many years afterwards, she, however, describes the disappointment she experienced on first meeting her father. Her imagination had sketched a fancy picture of him. She "had heard him spoken of as an officer, and had attached to this an idea of the warrior with a martial look, possibly the sword, if not the gun, by his side." All this disappeared when she saw him. He did not strike her as handsome, or even agreeable,—a result in part due to the fact that he had been ill and was very thin and pale. She speaks, however, of his laughter "quite from the heart," while the expression of his mouth, with teeth described as "the most finished pearls," was sweetness itself. He did not seem to manage her very successfully. She had little knowledge of the world, and her purchases in London he thought both extravagant and extraordinary. After having, by due discipline, learnt how to make an English courtesy, to the horror of her father, almost the first use she made of her newly acquired accomplishment was to courtesy to a housekeeper.

His labours in the production of cheap and nutritive food necessarily directed Rumford's attention to fireplaces and chimney flues. When he first published his essay on this subject in London, he reported that he had not less than five hundred smoky chimneys on his hands. His aid and advice were always ready, and were given indiscriminately to all sorts and conditions of men. Devonshire House, Sir Joseph Banks', the Earl of Bessborough's, Countess Spencer's, Melbourne House, Lady Templeton's, Mrs. Montagu's,

Lord Sudley's, the Marquis of Salisbury's, and a hundred and fifty other houses in London were placed in his care. The saving of fuel, with gain instead of loss of warmth, varied in these cases from one-half to two-thirds. "Giving very simple and intelligible information, about the philosophical principles of combustion, ventilation, and draughts, he prepared careful diagrams to show the proper measurements, disposal, and arrangements of all the parts of a fire-place and flue. He took out no patent for his inventions, but left them free to the public. In a poem published at this time by Thomas James Matthias we have the following appreciative reference to the labours of Rumford:—

> "Nonsense, or sense, I'll bear in any shape
> In gown, in lawn, in ermine, or in crape:
> What's a fine type, where truth exerts her rule?
> Science is science, and a fool's a fool.
> Yet all shall read, and all that page approve,
> When public spirit meets with public love.
> Thus late, where poverty with rapine dwelt,
> Rumford's kind genius the Bavarian felt,
> Not by romantic charities beguiled,
> But calm in project, and in mercy mild;
> Where'er his wisdom guided, none withstood,
> Content with peace and practicable good;
> Round him the labourers throng, the nobles wait,
> Friend of the poor, and guardian of the state."

The pall of smoke which habitually hung over London, "covering all its prominent edifices with a dingy and sooty mantle," curiously and anxiously interested him. He "saw in that smoke the unused material which was turned equally to waste and made a means of annoyance and insalubrity." He would bind himself, if the opportunity were allowed him, "to prove that from the heat, and the material of heat, which were thus wasted, that he would cook all the food used in the city, warm every apartment, and perform all the mechanical work done by means of fire." Under heat Rumford would doubtless comprise both the imperfectly consumed gases such as carbonic oxide, and the heated air and other gases discharged by the chimneys.

There is no doubt that the present age has entered largely into the labours of Rumford. Many of the devices and conveniences now employed in our kitchens owe their origin to him. The practical needs and mechanical ingenuity of his own countrymen have caused them to follow his lead with conspicuous success. We have, for example, in our modest little kitchen in the Alps, an American oven, which, with the expenditure of an extremely small amount of fire-wood, heats our baths, cooks our meat, bakes our bread, boils our clothes, and contributes to the warmth and comfort of the house. This arrangement traces its pedigree to Rumford.

In 1796 he founded the historic medal which bears his name. On the 12th of July of that year he wrote thus to Sir Joseph

Banks, then President of the Royal Society: "I take the liberty to request that the Royal Society would do me the honour to accept of £1000 stock in the Funds of this country, which I have actually purchased, and which I beg leave to transfer to the President, Council, and Fellows of the Royal Society, to the end that the interest of the same may be by them, and by their successors, received from time to time for ever, and the amount of the same applied and given once every second year, as a premium to the author of the most important discovery or useful improvement which shall be made, or published by printing, or in any way made known to the public, in any part of Europe, during the preceding two years, on Heat or Light."

He adds in a subsequent letter, as further defining his wishes, that the premium should be limited to new discoveries tending to improve theories of Fire, of Heat, of Light, and of Colours, and to new inventions and contrivances by which the generation, and preservation, and management of heat and of light may be facilitated. The device and inscriptions on the medal were determined by a committee. It was resolved "that the diameter of the medal do not exceed three inches, and that Mr. Milton be employed in sinking the dies of the said medal." Two medals are always given, one of gold, the other of silver, and a sum of about seventy pounds usually accompanies the medals. Rumford himself was the first recipient of the medal. The second was given to Sir John Leslie, the founder's celebrated rival in the domain of radiant heat. On the same date Rumford presented to the American Academy of Arts and Sciences the same sum for the promotion of the same object. In fact the letters to Sir Joseph Banks and to the Honourable John Adams, then President of the American Academy, are identical in terms. For a long series of years the American Academy did not consider that the candidates for the medal had reached the level of merit which would justify its award. No award was therefore made; and in 1829 the Rumford bequest had increased from five thousand to twenty thousand dollars. After some litigation the terms of the bequest were extended to embrace applications of it far beyond the design of the testator. Permission was obtained to apply the fund to the publication of books, or methods of discovery, bearing on the Count's favourite subjects of experiment; and to the aid and reward of scientific workers. Thus, in 1839, Dr. Hare, of Philadelphia, received from the Academy six hundred dollars for his invention of the compound blow-pipe, and his improvements in galvanic apparatus. In 1862 the Rumford medal was awarded to Mr. John B. Ericsson for his caloric engine; while Mr. Alvan Clark, so celebrated for his improvements of the refracting telescope, and the eminent Dr. John Draper of the University of New York, have been also numbered among the recipients.

Accompanied by his daughter, Rumford returned to Germany in 1796. "Three weeks' constant travel; circuitous routes to avoid troops; bad roads; still worse accommodations; passing nights in

the carriages for the want of an inn: scantiness of provisions, joined with great fatigue, rendered our journey by no means agreeable." At Munich they were lodged in the splendid house allotted to the Count. France and Austria were then at war, while Bavaria sought to remain rigidly neutral. Eight days after Rumford's arrival, the Elector took refuge in Saxony. Moreau had crossed the Rhine and threatened Bavaria. After a defeat by the French, the Austrians withdrew to Munich, but found the gates of the city closed against them. They planted batteries on a height commanding the city. According to arrangement with the Elector, Rumford assumed the command of the Bavarian forces, and by his firmness and presence of mind prevented either French or Austrians from entering Munich. A foreigner acting thus was sure to excite jealousy and encounter opposition, but, despite all this, he was eminently successful in realising his aims. The consideration in which he was held by the Elector is illustrated by the fact that he made Miss Thompson a Countess of the Empire, conferring on her a pension of 200*l.* a year, with liberty to enjoy it in any country where she might wish to reside.

The following incident is worth recording. In March 1796, Rumford's daughter, wishing to celebrate his birthday, chose out of his workhouse a dozen of the most industrious little boys and girls, dressed them up in the uniform of that establishment, and robing herself in white, led them into his room and presented them to him. He was so much touched by the incident, that he made her a present of two thousand dollars (400*l.*) on condition that she should, in her will, apply the interest of the sum to the clothing, every year for ever, on her own birthday, of twelve meritorious children—six girls and six boys—in the Munich uniform. The poor children were to be chosen from her native town, Concord. Habit must to some extent have blinded Rumford's eyes to the objection which independent New Englanders were likely to make to this fantastic apparel. They bluntly stated their objections, but "with grateful hearts" they nevertheless expressed their willingness to accept the donation. Nothing further was done during Rumford's lifetime.

The New England girl, brought up in Concord, transplanted thence to London, and afterwards to Munich, was subjected to a somewhat trying ordeal. After a short period of initiation, she appears to have passed through it creditably. Her writing does not exhibit her as possessing any marked qualities of intellect. She was bright, gossipy, "volatile," and throws manifold gleams on the details of Rumford's life. He constantly kept a box at the opera, though he hardly ever went there, and hired by the year a doctor named Haubenal. She amusingly describes a quintuple present made to her by her father soon after her arrival in Munich. The first item was "a little shaggy dog, as white as snow, excepting black eyes, ears and nose"; the second was a lady named Veratzy, who was sent to teach her French and music; the third was a Catholic priest, named Dillis, who was to be her drawing master; the fourth was a

teacher of Italian, named Alberti; and the fifth, the before-mentioned Dr. Haubenal, who was to look after her health. She did not at all like the arrangement. She was particularly surprised and shocked at a doctor's offering his services before they were wanted. "Said I to myself, surrounded by people who speak French—and all genteel people speak it at Munich—and knowing considerable of the language already, where is the use of my fatiguing myself with masters? Music the same." In fact the little dog "Cora" was the only welcome constituent of the gift.

She describes with considerable spirit a ball which was organised to celebrate her father's birthday. All united to do him honour. Wreaths surrounded his bust; his workhouse children, joined by some children of the nobility, all dressed in white, handed addresses to him, and sang in accompaniment to the swell of music of which he was passionately fond. All this was arranged without his knowledge, and possibly not without an intention to give dramatic force to a revelation made at the time. It was observed that Rumford had singled out from the children a little girl of eight, who accompanied him when he walked, and took her place beside him when he sat. The little girl was his illegitimate child. Sarah, on learning this, was stunned, threw herself into the dance that she had previously declined, and thus whirled away her indignation. Her partner was the young Count Taxis, Rumford's aide-de-camp, between whom and Rumford's daughter a friendly intimacy was obviously growing up. Rumford noticed this, and disapproved of it. Being invited to dinner at the house of the Countess Lerchenfeld, with her father's consent Miss Thompson went. Count Taxis happened to be one of the party, and on hearing this Rumford jumped to the conclusion that a ladies' conspiracy was afoot to counteract his wishes. With a lowering look he taxed his daughter with what he supposed to be an intrigue. At first she could only stare at him in surprise. "After which, on knowing what it meant, like many young people who laugh when there is nothing to laugh at, an irresistible inclination seized me to laugh." She gave way to her inclination, "and it ended in my father's boxing my ears." She was stunned by the indignity and "quitted the room, without making an observation, or trying to appease him by saying I was innocent."

The elector put the seal to his esteem for Rumford by appointing him as Plenipotentiary from Bavaria to the Court of London. King George, however, declined to accept him in this capacity. Mr. Paget, the minister at the court of Bavaria, was desired "to lose no time in apprising the ministers of His Electoral Highness that such an appointment would be by no means agreeable to His Majesty, and that His Majesty relies therefore on the friendship and good understanding which has always hitherto subsisted between himself and the Elector of Bavaria, and that His Highness will have no hesitation in withdrawing it." The King had made up his mind. "Should there unexpectedly arise any difficulty about a compliance with the re-

quest, which His Majesty is so clearly warranted in making, I am to direct you, in the last resort, to state in distinct terms that His Majesty will by no means consent to receive Count Rumford in the character which has been assigned to him." The fact of Rumford's being not only a British subject, but that he had actually filled a confidential situation under the British Government, were cited, as rendering his appointment peculiarly objectionable. Some correspondence ensued between Lord Grenville and Rumford, but the appointment was not ratified.

Rumford was obviously stung by the refusal of King George to accept him as Bavarian minister; and the thought which had often occurred to him of returning to his native country now revived. Mr. Rufus King was at that time American Ambassador in London; and he, by Rumford's desire, wrote to Colonel Pickering, then Secretary of State for the United States, informing him that intrigues in Bavaria, and the refusal of the English king, had caused the Count to decide on establishing himself at, or near, Cambridge, Massachusetts. Mr. King describes the Count's intention to live in the character of a German nobleman, renouncing all political action, and devoting himself to literary pursuits. Mr. King describes Rumford as having had much experience of cannon foundries; and as having made important improvements in the mounting of flying artillery. He was, moreover, the possessor of an extensive military library, and wished nothing more ardently than to be useful to his native country. They had made provision for the institution of a military academy in the United States. This they offered to place under the superintendence of Rumford. "I am authorised," said Mr. King, "to offer you, in addition to the superintendence of the military academy, the appointment of Inspector General of the Artillery of the United States; and we shall moreover be disposed to give to you such rank and emoluments as would be likely to afford you satisfaction, and to secure to us the advantage of your service."

The hour for the final decision approached, but before it arrived another project had laid hold of Rumford's imagination—a project which in its results has proved of more importance to science, and of more advantage to mankind, than any which this multifarious genius had previously undertaken. This project was the foundation of the Royal Institution of Great Britain. In answer to the American Ambassador, he says, "Nothing could have afforded me so much satisfaction as to have had it in my power to have given to my liberal and generous countrymen such proof of my sentiments as would in the most public and ostensible manner have evinced, not only my gratitude for the kind attentions I have received from them, but also the ardent desire I feel to assist in promoting the prosperity of my native country. But engagements, which great obligations have rendered sacred and inviolable, put it out of my power to dispose of my time and services with that unreasoned freedom which would be necessary in order to enable me to accept of those generous offers

which the Executive Government of the United States has been pleased to propose to me."

The climate of Europe did not seem to suit the Countess Sarah. Possibly the simple tastes and habits of her childhood were too deeply ingrained in her constitution to permit of her deriving any real enjoyment from the outsided, and apparently noisy life, which she was forced to lead in Munich and London. Be this as it may, she returned to America, reaching the port of Boston on October 10th, 1799, "being then just twenty-five years of age." Rumford himself remained in England with the view of realising what I have called the greatest project of his life—the founding of the Royal Institution.

His ideas on this subject took definite shape in 1799. They were set forth in a pamphlet of fifty pages bearing the following lengthy title: "Proposals for forming by subscription, in the metropolis of the British Empire, a public institution for diffusing the knowledge and facilitating the general introduction of useful mechanical inventions and improvements, and for teaching, by courses of philosophical lectures and experiments, the application of science to the common purposes of life." The introduction to this pamphlet is dated from Rumford's residence in Brompton Row, March 4th, 1799. His aim he alleges to be to cause science and art to work together; to establish relations between philosophers and workmen; to bring their united efforts to bear in the improvement of agriculture, manufactures, commerce, and in the augmentation of domestic comforts. He specially dwells on the management of fire, it being, as he thinks, a subject of peculiar interest to mankind. Fuel, he asserted, cost the kingdom more than ten millions sterling annually, which was much more than twice what it ought to cost. Rumford knew human nature well, and for the greater portion of his life knew how to appeal to it with effect. In fact, the knowledge never failed him, though towards the end irritability, due to ill-health and crosses of various kinds, rendered him less able to apply the knowledge than when he was in the blossom of his prime. As regards the success of his new scheme, he urged upon the excellent men with whom he acted the necessity of making the indolent and luxurious take an interest in it. Such persons, he says, "must either be allured or shamed into action." Hence, he urges, the necessity of making benevolence "fashionable."

It ought to be mentioned that Rumford at this time could count on the sympathy and active support of a number of excellent men, who, in advance of him, had founded a "Society for bettering the condition and increasing the comforts of the poor." Rumford sought the aid of the committee of this society. It was agreed on all hands that the proposed new Institution promised to be too important to permit of its being made an appendage to any other. It was resolved that it should stand alone. A committee consisting of eight members of the above society was, however, appointed to confer with Rumford regarding his plan. To each member of this committee he

submitted a statement of his views. These are in part set forth in the title to his pamphlet already quoted. The aim of the institution, furthermore, was "to excite a spirit of improvement among all ranks of society, and to afford the most effectual assistance to those who are engaged in the various pursuits of useful industry." He begs, however, that His Majesty's Ministers may be informed of the intention of the founders of the Institution to accept his services. This he deems necessary because of his being, in the first place, a subject of His Majesty, and also, by His Majesty's special permission, the servant of a foreign prince. He recommends that the Government should be fully informed not only as to the general aims but also of the details of the scheme, and then asked for countenance and support in carrying it into execution.

The committee met and ratified Rumford's proposals. They agreed that subscribers of fifty guineas each should be the perpetual proprietors of the Institution; that a contribution of ten guineas should secure the privileges of a life subscriber; whilst a subscription of two guineas should constitute an annual subscriber. Besides other important rights, each proprietor was to receive two transferable tickets, admitting to every part of the Institution, and to all the lectures and experiments. Each life subscriber was to receive one ticket, not transferable, securing free admission to every part of the establishment, and to all lectures and experiments. An annual subscriber had a single ticket for a single year, but might at any time become a life subscriber by the additional payment of eight guineas. The managers, nine in number, were to be chosen by ballot by the proprietors. The managers were to be unpaid and, without any pecuniary advantage to themselves, were held solemnly pledged to the faithful discharge of their duties. Three were to constitute a quorum, but in special cases six were required. A Committee of Visitors was also appointed, the same in number as the Committee of Managers, and holding office for the same number of years.

The managers were to devote the surplus funds of the Institution to the purchase of models of inventions, and improvements in the mechanical arts; and a room in the Institution was to be devoted to the reception of them. The room still exists, and though diverted from its original purpose, is called "the Model Room." A general meeting of the proprietors was held at the house of Sir Joseph Banks, in Soho Square, on the 7th of March, 1799. Fifty-eight persons, comprising men of distinction in science, members of Parliament and of the nobility, including one bishop, were found to have qualified as proprietors by the subscription of fifty guineas each. The prelate was the Bishop of Durham. The Committee of Managers was chosen, and they held their first meeting at the house of Sir Joseph Banks on the 9th of March, 1799. Mr. Thomas Bernard, one of the most active members of the Society from whose committee the first managers were chosen, was appointed Secretary. To Rumford and Bernard was delegated the duty of preparing a draught of a charter; while

Earls Morton and Spencer, Sir Joseph Banks, and Mr. Pelham, were requested "to lay the proposals before His Majesty, the Royal Family, the Ministers, the great officers of State, the members of both Houses of Parliament, of the Privy Council, and before the twelve judges."

On the 13th of January, 1800, the Royal Seal was attached to the Charter of the Institution. In the same year was published, in quarto form, 'The Prospectus, Charter, Ordinance, and Bye-laws of the Royal Institution of Great Britain.' The king was its Patron, and the first officers of the Institution were appointed by him. The Earl of Winchilsea was President. Lord Morton, Lord Egremont, and Sir Joseph Banks were Vice-Presidents. The managers, chosen by sealed ballot by the proprietors, were divided into three classes of three each; the first class serving for one, the second for two, and the third for three years. The Earls of Bessborough, Egremont, and Morton, respectively, headed the lists of the three classes. Rumford himself was appointed to serve for three years. The three lists of Visitors were headed by the Duke of Bridgewater, Viscount Palmerston and Earl Spencer, respectively. That Rumford possessed the power of persuasion, and the infection of enthusiasm, is sufficiently demonstrated by this powerful list. But neither persuasion nor enthusiasm might have been found availing had not his actual achievements in Bavaria occupied the background. The first Professor of Natural Philosophy and Chemistry was Dr. Thomas Garnett, while the first Treasurer was Mr. Thomas Bernard. But this was not enough. A home and foreign secretary, legal counsel, a solicitor and a clerk, were added to the list. One rule established at this time has been adhered to with great fidelity to the present day. No political subject was to be mentioned in the lectures.

In a somewhat florid style Rumford (for he was obviously the writer) descants on the name and objects of the new project. The word Institution is chosen because it had been least used previously, and because it best indicates the objects of the new society. The influence of the mechanical arts on the progress of civilisation and refinement is pointed out and illustrated by reference to nations, provinces, towns, and even villages which thrive in proportion to the activity of their industry. "Exertion quickens the spirit of invention, makes science flourish, and increases the moral and physical powers of man." The printing-press, navigation, gunpowder, the steam-engine, are referred to as having changed the whole course of human affairs. The slowness with which improvements make their way among workmen is ascribed to the influence of habit, prejudice, suspicion, jealousy, dislike of change, and the narrowing effect of the subdivision of work into many petty occupations. But slowness is also due to the greed for wealth, the desire for monopoly, the spirit of secret intrigue exhibited among manufacturers. Between these two the philosopher steps in, whose business it is "to examine every operation of nature and art, and to establish general theories for

the direction and conducting of future processes." But philosophers may become dreamers, and they have therefore habitually to be called back to the study of practical questions which bear upon the ordinary pursuits of life. Science and practice are, in short, to interact, to the advantage of both. This object may be promoted by the offering of premiums, as done by the Society of Arts,* by the granting of patents; and, finally, by the method of the new Institution—the diffusion of the knowledge of useful mechanical inventions, and their introduction into life.

One of the first practical steps taken towards the realisation of these ideas was the purchase of the house, or rather houses, in Albemarle Street, in which we are now assembled, and their modification to suit the objects in view. Rumford's obvious intention was to found an Institute of Technology and Engineering. Mere description was not sufficient. He demanded something visible and tangible, and therefore proposed that the Institution should be made a repository for models of all useful contrivances and improvements: cottage fireplaces and kitchen utensils; kitchens for farm-houses and for the houses of gentlemen; a laundry, including boilers, washing, ironing, and drying-rooms; German, Swedish, and Russian stoves; open chimney fireplaces, with ornamental grates; ornamental stoves; working models "of that most curious and most useful machine, the steam-engine;" brewers' boilers; distillers' coppers; condensers; large boilers for hospitals; ventilating apparatus in hot-houses; lime-kilns; steam-boilers for preparing food for stall-fed cattle; spinning-wheels; looms; agricultural implements; bridges of various constructions; human food; clothing; houses; towns; fortresses; harbours; roads; canals; carriages; ships; tools; weapons; &c. Chemistry was to be applied to soils, tillage, and manures; to the making of bread, beer, wine, spirits, starch, sugar, butter, and cheese; to the processes of dyeing, calico-printing, bleaching, painting, and varnishing; to the smelting of ores; the formation of alloys; to mortars, cements, bricks, pottery, glass, and enamels. Above all, "the phenomena of *light* and *heat*—those great powers which give life and energy to the universe—powers which, by the wonderful process of combustion, are placed under the command of human beings—will engage a profound interest." In reference to the alleged size of the bed of Og, the king of Basan, Bishop Watson proposed to Tom Paine the problem to determine the bulk to which a human body may be augmented before it will perish by its own weight. As regards the projected Institution, Rumford surely had passed this limit, and by the ponderosity of his scheme had ensured either the necessity of change or the certainty of death. In such an establishment Davy was sure to be an iconoclast. He cared little for models—not even for the apparatus with which his own best discoveries were

* Founded in 1753.

made, but incontinently broke it up whenever he found it could be made subservient to further ends.

The Journal of the Royal Institution was established at this time, and published under Rumford's direction. No private advertisements were to appear in it, but it was to be sold for 3*d.* when its contents amounted to eight pages, and for 6*d.* when they amounted to sixteen. The experiments and experimental lectures of Davy were then attracting attention. Rumours of the young chemist reached Rumford through Mr. Underwood and Mr. James Thompson. At Rumford's request, Davy came to London. His life at the moment was purely a land of promise, but Rumford had the sagacity to see the promise, and the wisdom to act upon his insight. Nor was his judgment rapidly formed; for several interviews, doubtless meant to test the youth, preceded his announcement to Davy, on the 16th of February, 1801, the resolution of the managers, "That Mr. Humphry Davy be engaged in the service of the Royal Institution, in the capacity of Assistant Lecturer in Chemistry, Director of the Chemical Laboratory, and Assistant Editor of the Journals of the Institution; and that he be allowed to occupy a room in the house, and be furnished with coals and candles, and that he be paid a salary of one hundred guineas per annum." Rumford, moreover, held out to Davy the prospect, if he devoted himself entirely and permanently to the Institution, of becoming, in the course of two or three years, full Professor of Chemistry, with a salary of 300*l.* per annum, "provided," he adds, "that within that period you shall have given proofs of your fitness to hold that distinguished situation." This promise of the professorship in two or three years was ominous for Dr. Garnett, between whom and the managers differences soon arose which led to his withdrawal from the Institution.

Davy began his duties on Wednesday, the 11th of March, 1801. He was allowed the room adjoining that occupied by Dr. Garnett, to whom he was to refund the expenses incurred in furnishing the room. The committee of expenditure paid to Dr. Garnett 20*l.* 2*s.* 3*d.* for a new Brussels carpet, and 17*l.* 6*s.* for twelve chairs, the carpet and chairs being transferred to the room occupied by the managers. "Count Rumford reported further that he had purchased cheaper a second-hand carpet for Mr. Davy's room, together with such other articles as appeared to him necessary to render the room habitable, and among the rest a new sofa-bed, which, in order that it may serve as a model for imitation, has been made complete in all its parts."

The name of a man who has no superior in its annals now appears for the first time in connection with the Institution. Here also the sagacity of Rumford was justified by events. At the suggestion of Sir Joseph Banks he had an interview with Dr. Thomas Young, destined to become so illustrious as the decipherer of the Egyptian hieroglyphics, and, by his discovery of Interference, the founder of the undulatory theory of light. It was proposed to him, by Rumford, to accept an engagement as Professor of Natural Philosophy in the

Institution, as Editor of its Journals, and as superintendent of the house, at a salary of 300*l.* per annum. Young accepted the appointment, and the Managers confirmed it by resolution on the 3rd of August, 1801:—" *Resolved*, that the managers approve of the measures taken by Count Rumford; and that the appointment of Dr. Young be confirmed."

Rumford's health fluctuated perpetually, and it was said at the time that this was due in some measure to the fanciful notions he entertained, and acted on, with regard to diet and exercise. But Dr. Young affirms that his habits in these respects were guided by his physicians.

Many years ago, wishing to supplement my knowledge of the Turkish bath, I referred to a paper of Rumford's which gave an account of a visit to Harrogate and his experience there. According to the rules of the place he had his bath in the evening, and went to bed immediately afterwards. He found himself restless and feverish; the bath, indeed, seemed to do him more harm than good. An observant fellow-lodger of his had had, and had corrected, the same experience. Acting on his advice, Rumford took his bath two hours before his dinner, engaging afterwards in his usual work, or going out to have a blow on the common. So far from suffering from chill through this exposure, he found himself invigorated by it. My own experience, I may say, corroborates all this. Rumford took the senses of man as he found them, and tried to enhance the gratifications thence derived:—" To increase the pleasure of a warm bath he suggests the burning of sweet scented woods, and aromatic gums and resins in small chafing dishes in the bathing rooms, by which the air will be perfumed with the most pleasant odours." He spiritedly defends this counsel:—" Effeminacy is no doubt very despicable, especially in a person who aspires to the character and virtues of a man. But I see no cause for calling anything effeminate which has no tendency to diminish either the strength of the body, the dignity of the sentiments, or the energy of the mind. I see no good reason for considering those grateful aromatic perfumes, which in all ages have been held in such high estimation, as a less elegant or less rational luxury than smoking tobacco or stuffing the nose with snuff."

Rumford, for a year or so, occupied rooms in the Institution, but his private residence was in Brompton Row, described by his friend Pictet as being about a mile from London. Grass and trees grew in front of the house. The windows had a double glazing, and outside were placed vases of flowers and odorous shrubs. Pictet, who was Rumford's guest in 1801, minutely describes the whole arrangement of the house. Into Rumford's working room, which overlooked the country, the light came through a set of windows arranged on the arc of a circle. The window-sills were arranged with flowers and shrubs, so that you might suppose yourself to be in the country, close to a garden bordered by a park. Pictet goes on to describe the various strokes of ingenuity shown in the management of fuel and

fire-places. The beds, moreover, were disguised as elegant sofas. Under each sofa were two deep drawers containing the bedding and other night gear, all of which were hidden by a fringed valence. At night the sofa was converted in a few minutes into an excellent bed, while in the morning, with equal rapidity, the bed was transformed into an ornamental piece of furniture. Pictet occupied one-half of the charming dwelling. Perfect freedom was given and enjoyed, and the learned Genevese always tried to arrange his day's work so that he might, if possible, engage his friend on some subject of research common to them both.

A portion of the motive force of a man of Rumford's temperament may be described as irritability. During the possession of physical vigour and sound health, this force is clasped by power of will and directed by intelligence and tact. But when health slackens and physical vigour subsides, what was formerly a firmly ruled power becomes an energy wanting adequate control. Rumford's success in managing all manner of men in Bavaria illustrates his pliancy as much as his strength. But before he started the Royal Institution his health had given way, and his irritability, it is to be feared, had got the upper hand. In point of intellect, moreover, he came then into contact with people of larger calibre and more varied accomplishments than he had previously met. He could hardly count upon the entire sympathy of Young and Davy, though I believe he remained on friendly terms with them to the end. They were gems of a different water, if I may use the term, from Rumford. The chief object of his fostering care was mechanical invention, as applied to the uses of life. The pleasures of both Young and Davy lay in another sphere. To them science was an end, not a means to an end. The getting at the mind of nature, and the revealing of that mind in great theories, were the objects of their efforts, and formed the occupation of their lives. Had they been as enthusiastic as Rumford himself in Rumford's own direction, the three united would probably have daunted opposition, and for a somewhat longer time endeavoured to realise Rumford's dream. But differences arose between him and the other managers. "It is very clear to me," writes Dr. Bence Jones to Dr. Ellis, "that Count Rumford fell out with Mr. Bernard and with Sir John Hippesley. The fact was that Rumford's idea of workshops and kitchen, industrial school, mechanics' institution, model exhibition, social club-house, and scientific committees to do everything, was much too big and unworkable for a private body, and was fitted only for an absolute wealthy government." In 1803 Dr. Bence Jones informs us that difficulties were gathering round the Institution, and it was even proposed to sell it off. Rumford had quitted London and gone to Paris. By Davy's aid, Mr. Bernard and Sir John Hippesley carried on the work, but in a fashion different from that contemplated by Rumford—that is to say, "without workshops, or mechanics' institute, or kitchen, or model exhibition." The place of these was taken by experimental

and theoretical researches, which, instead of dealing with things already achieved, carried the mind into unexplored regions of nature, forgetful, if not neglectful, whether the discoveries made in that region had or had not a bearing on the arts and comforts or necessities of material life.

Rumford and his Institution had to bear the brunt of ridicule, and he felt it; but men of ready wit have not abstained from exercising it on societies of greater age and higher claims. Shafts of sarcasm without number have been launched at the Royal Society. It is perfectly natural for persons who have little taste for scientific inquiry and less knowledge of the methods of nature, to feel amused, if not scandalised, by the apparently insignificant subjects which sometimes occupy the scientific mind. They are not aware that in science the most stupendous phenomena often find their suggestion and interpretation in the most minute,—that the smallest laboratory fact is connected by indissoluble ties with the grandest operations of nature. Thus the iridescences of the common soap-bubble, subjected to scientific analysis, have emerged in the conclusion that stellar space is a *plenum* filled with a material substance, capable of transmitting motion with a rapidity which would girdle the equatorial earth eight times in a second; while the tremors of this substance, in one form, constitute what we call light, and, in all forms, constitute what we call radiant heat. Not seeing this connection between great and small; not discerning that as regards the illustration of physical principles there is no great and no small, the wits, considering the small contemptible, permitted sarcasm to flow accordingly. But these things have passed away, otherwise it would not be superfluous to remind this audience, as a case in point, that the splendour which in the form of the electric light now falls upon our squares and thoroughfares, has its germ and ancestry in a spark so feeble as to be scarcely visible when first revealed within the walls of this Institution.

It is with reluctance that I take the slightest exception to what my American friends have written regarding Rumford and his achievements. But what they have written induces me to assure them that the scientific men of England are not prone to stinginess in recognising the merits of their fellow-labourers in other lands; and had Rumford, instead of accomplishing none of his work in the land of his birth, accomplished the whole of it there, his recognition among us here would not be less hearty than it is now. As things stand, national prejudice, if it existed, might be expected to lean to Rumford's side. But no such prejudice exists, and to write as if it did exist is a mistake. In reference to myself, Dr. Ellis, gently but still reproachfully, makes the following remark:—"Professor Tyndall in his work on Heat has but moderately recognised the claims and merit of Rumford, when, after largely quoting from his essay, he adds, 'When the history of the dynamical theory of heat is written, the

man who in opposition to the scientific belief of his time could experiment, and reason upon experiment, as did Rumford in the investigation here referred to, cannot be lightly passed over.'" In my opinion, the most dignified and impressive way of dealing with labours like those of Rumford, is to show by simple quotations, well selected, what their merits are. This I did in the book referred to by Dr. Ellis, which was published at least eight years in advance of his. But the expression of my admiration for Rumford was not confined to the passage above quoted, which is taken from the appendix to one of my lectures. In that lecture I drew attention to Rumford's labours in the following words:—"I have particular pleasure in directing the reader's attention to an abstract of Count Rumford's memoir on the generation of heat by friction, contained in the appendix to this lecture. Rumford in this memoir annihilates the material theory of heat. Nothing more powerful on the subject has since been written."

But I must not go too far, nor suffer myself to deal with one-sided exclusiveness with the merits of Rumford. The theoretic conceptions with which he dealt were not his conceptions, but had been the property of science long prior to his day. This, I fear, was forgotten when the following claim for Rumford was made by a writer who has done excellent service in diffusing sound science among the people of the United States:—"He was the man who first took the question of the nature of heat out of the domain of metaphysics, where it had been speculated upon since the time of Aristotle, and placed it upon the true basis of physical experiment." The writer of this passage could hardly, when he wrote it, have been acquainted with the experiments and the reasonings of Boyle and Hooke, and Leibnitz and Locke. As regards the nature of heat, these men were quite as far removed from metaphysical subtleties as Rumford himself. They regarded heat as "a very brisk agitation of the insensible parts of an object which produces in us that sensation from whence we denominate the object hot; so what in our sensation is heat, in the object is nothing but motion." Locke, from whom I here quote, and who merely expresses the ideas previously enunciated by Boyle and Hooke, gives his reasons for holding this theoretic conception. "This," he says, "appears by the way heat is produced, for we see that the rubbing of a brass nail upon a board will make it very hot; and the axle-trees of carts and coaches are often hot, and sometimes to a degree that it sets them on fire, by the rubbing of the naves of the wheels upon them. On the other side, the utmost degree of cold is the cessation of that motion of the insensible particles which to our touch is heat." The precision of this statement could not, within its limits, be exceeded at the present day.

There is a curious resemblance, moreover, between one of the experiments of Boyle, and the most celebrated experiment of Rumford. Boyle employed three men accustomed to the work, to hammer nimbly

a piece of iron. They made the metal so hot, that it could not be safely touched. As in the case of Rumford, people were looking on at this experiment, and Boyle's people, like those of Rumford, were struck with wonder, to see the sulphur of gunpowder ignited by heat produced without any fire. Hooke is equally clear as regards the nature of heat, and, like Rumford himself, but more than a century before him, he compares the vibrations of heat with sonorous vibrations. That Rumford went beyond these men is not to be denied. It could not be otherwise with a spirit so original and penetrating. But to speak of the space between him and Aristotle as if it were a scientific vacuum is surely a mistake.

While in Paris, Rumford made the acquaintance of Madame Lavoisier, a lady of wealth, spirit, social distinction, and, it is to be added, a lady of temper. Her illustrious husband had suffered under the guillotine on the 8th of May, 1794; and inheriting his great name, together with a fortune of three million francs, she gathered round her, in her receptions, the most distinguished society of Paris. She and Rumford became friends, the friendship afterwards passing into what was thought to be genuine affection. The Elector of Bavaria took great interest in his projected marriage, and when that consummation came near, settled upon him an annuity of 4000 florins. Before their marriage he was joined by Madame Lavoisier at Munich, whence they made a tour to Switzerland together. In a letter to his daughter he thus describes his bride elect: "I made the acquaintance of this very amiable woman in Paris, who, I believe, would have no objection to having me for a husband, and who in all respects would be a proper match for me. She is a widow without children, never having had any; is about my own age (she was four years younger than Rumford), enjoys good health, is very pleasant in society, has a handsome fortune at her own disposal, enjoys a most respectable reputation, keeps a good house, which is frequented by all the first philosophers and men of eminence in the science and literature of the age, or rather of Paris. And, what is more than all the rest, is goodness itself." He goes on to describe her as having been very handsome in her day, "and even now at forty-six or forty-eight is not bad looking." He describes her as rather *embonpoint*, with a great deal of vivacity, and as writing incomparably well.

Before the marriage could take place, he was obliged to obtain from America certificates of his birth, and of the death of his former wife. All preliminaries having been arranged, Count Rumford and Madame Lavoisier were married in Paris on the 24th of October, 1805. He describes the house in which they lived, Rue d'Anjou, No. 39, as a paradise. "Removed from the noise and bustle of the street, facing full to the south, in the midst of a beautiful garden of more than two acres, well planted with trees and shrubbery. The entrance from the street is through an iron gate by a beautiful winding avenue well planted, and the porter's lodge is by the side of

this gate, a great bell to be rung in case of ceremonious visits." Long after this event Rumford's daughter commented on it thus:— "It seems there had been an acquaintance between these parties of four years before the marriage. It might be thought a long space of time for perfect acquaintance. But, 'ah Providence! thy ways are past finding out.'"

In a letter written to his daughter two months after his marriage, he describes their style of living as really magnificent; his wife as exceedingly fond of company, in the midst of which she makes a splendid figure. She seldom went out, but kept open house to all the great and worthy. He describes their dinners and evening teas, which must have been trying to a man who longed for quiet. He could have borne the dinners, but the teas and their gossip annoyed him. Instead of living melodious days, his life gradually became a discord; and on the 15th of January, 1806, he confides to his daughter, as a family secret, that he is "not at all sure that two certain persons were not wholly mistaken in their marriage, as to each other's characters." The dénouement hastened; and on the first anniversary of his marriage he writes thus to his daughter:—"My dear child. This being the first year's anniversary of my marriage, from what I wrote two months after it you will be curious to know how things stand at present. I am sorry to say that experience only serves to confirm me in the belief that in character and natural propensities Madame de Rumford and myself are totally unlike, and never ought to have thought of marrying. We are besides, both too independent, both in our sentiments and habits of life, to live peaceably together,—she having been mistress all her days of her actions, and I, with no less liberty, leading for the most part the life of a bachelor. Very likely she is as much disaffected towards me as I am towards her. Little it matters with me, but I call her a female dragon,—simply by that gentle name! We have got to the pitch of my insisting on one thing and she on another."

On the second anniversary of his marriage, matters were worse. The quarrels between him and Madame had become more violent and open, and having used the word quarrels to his daughter, he gives the following sample of them:—"I am almost afraid to tell you the story, my good child, lest in future you should not be good; lest what I am about relating should set you a bad example, make you passionate, and so on. But I had been made very angry. A large party had been invited I neither liked nor approved of, and invited for the sole purpose of vexing me. Our house being in the centre of the garden, walled around, with iron gates, I put on my hat, walked down to the porter's lodge, and gave him orders, on his peril, not to let any one in. Besides, I took away the keys. Madame went down, and when the company arrived, she talked with them,—she on one side, they on the other, of the high brick wall. After that she goes and pours boiling water on some of my beautiful flowers."

Six months later on, the sounds of lamentation and woe are con-

tinued. There was no alteration for the better. He thought of separation, but the house and garden in the Rue d'Anjou being a joint concern, legal difficulties arose as to the division of it. "I have suffered," he says to his daughter, "more than you can imagine for the last four weeks; but my rights are incontestable and I am determined to maintain them. I have the misfortune to be married to one of the most imperious, tyrannical, unfeeling women that ever existed, and whose perseverance in pursuing an object is equal to her profound cunning and wickedness in framing it." He purposed taking a house at Auteuil. It would be unfortunate if, notwithstanding all the bounties of the King of Bavaria, he could not live more independently than with this unfeeling, cunning, tyrannical woman. "Alas! little do we know people at first sight! Do you preserve my letters? You will perceive that I have given very different accounts of this woman, for *lady* I cannot call her." He describes his habitation as no longer the abode of peace. He breakfasts alone in his apartment, while to his infinite chagrin most of the visitors are his wife's determined adherents. He is sometimes present at her tea parties, but finds there little to amuse him. "I have waited," he says (which we may doubt), "with great, I may say unexampled patience, for a return of reason and a change of conduct, but I am firmly resolved not to be driven from my ground, not even by disgust. A separation," he adds, "is unavoidable, for it would be highly improper for me to continue with a person who has given me so many proofs of her implacable hatred and malice."

The lease of the villa at Auteuil was purchased by Rumford in 1808. The separation between him and his wife took place "amicably" on the 13th of June, 1809. Ever afterwards, however, anger rankled in his heart. He never mentions his wife but in terms of repugnance and condemnation. His release from her fills him with unnatural jubilation. On the fourth anniversary of his wedding-day he writes to his daughter, "I make choice of this day to write to you, in reality to testify joy, but joy that I am away from her." On the fifth anniversary he writes thus: "You will perceive that this is the anniversary of my marriage. I am happy to call it to mind that I may compare my present situation with the three and a half horrible years I was living with that tyrannical, avaricious, unfeeling woman." The closing six months of his married life he describes as a purgatory sufficiently painful to do away with the sins of a thousand years. Rumford, in fact, writes with the bitterness of a defeated man. His wife retained her friends, while he, who, a short time previously, had been the observed of all observers, found himself practically isolated. This was a new and bitter experience, the thought of which, pressing on him continually, destroyed all magnanimity in his references to her.

From 1772 to 1800, Rumford's house at Auteuil had been the residence of the widow of a man highly celebrated in his day as a freethinker, but whom Lange describes as "the vain and superficial Helvetius." It is also the house in which in the month of January

1870, the young journalist Victor Noir was shot dead by Prince Pierre Bonaparte. Here, towards the end of 1811, the count was joined by his daughter. They found pleasure in each other's company, but the affection between them does not appear to have been intense. In his conversations with her the source of his bitterness appears. "I have not," he says, "deserved to have so many enemies; but it is all from coming into France, and forming this horrible connection. I believe that woman was born to be the torment of my life." The house and gardens were beautiful; tufted woods, winding paths, grapes in abundance, and fifty kinds of roses. Notwithstanding his hostility to his wife, he permitted her to visit him on apparently amicable terms. The daughter paints her character as admirable, ascribing their differences to individual independence arising from having been accustomed to rule in their respective ways: "It was a fine match, could they but have agreed." One day in driving out with her father, she remarked to him how odd it was that he and his wife could not get on together, when they seemed so friendly to each other, adding that it struck her that Madame de Rumford could not be in her right mind. He replied bitterly, "Her mind is, as it has ever been, to act differently from what she appears."

The statesman Guizot was one of Madame de Rumford's most intimate friends, and his account of her and her house is certainly calculated to modify the account of both given by her husband. Rumford became her guest at a time when he enjoyed in public "a splendid scientific popularity. His spirit was lofty, his conversation was full of interest, and his manners were marked by gentle kindness. He made himself agreeable to Madame Lavoisier. He accorded with her habits, her tastes, one might almost say with her reminiscences. . . . . She married him, happy to offer to a distinguished man a great fortune and a most agreeable existence." Guizot goes on to state that their characters and temperaments were incompatible. They had both grown to maturity accustomed to independence, which it is not always easy even for tender affection to stifle. The lady had stipulated, on her second marriage, that she should be permitted to retain the name of Lavoisier, calling herself Madame Lavoisier de Rumford. This proved disagreeable to the Count, but she was not to be moved from her determination to retain the name. "I have," she says, "at the bottom of my heart a profound conviction that M. de Rumford will not disapprove of me for it, and that on taking time for reflection, he will permit me to continue to fulfil a duty which I regard as sacred." Guizot adds that the hope proved deceptive, and that "after some domestic agitations, which M. de Rumford, with more of tact, might have kept from becoming so notorious, a separation became necessary." Guizot describes her dinners and receptions during the remaining twenty-seven years of her life as delightful. Cultivated intellects, piquant and serious conversation, excellent music, freedom of mind and tongue, without personal antagonism or political bias, "licence of thought

and speech without any distrust or disquiet as to what authority might judge or say—a privilege then more precious than any one to-day imagines, just as one who has breathed under an air-pump can best appreciate the delight of free respiration." One cannot, however, forget the pouring of boiling water over the roses.

The 'Gentleman's Magazine' for 1814 describes the seclusion in which Rumford's later days were spent. After the death of the illustrious Lagrange, he saw but two or three friends, nor did he attend the meetings of the National Institute, of which he was a member. Cuvier was then its perpetual secretary, and for him Rumford always entertained the highest esteem. He differed from Laplace on the question of surface-tension, and dissent from a man then standing so high in the mathematical world was probably not without its penal consequences. Rumford always congratulated himself on having brought forward two such celebrated men as the Bavarian general Wieden, who was originally a lawyer or land steward, and Sir Humphry Davy. The German, French, Spanish, and Italian languages were as familiar to the count as English. He played billiards against himself; he was fond of chess, which however made his feet like ice and his head like fire. The designs of his own inventions were drawn by him with great skill; but he had no knowledge of painting and sculpture, and but little feeling for them. He had no taste for poetry, but great taste for landscape gardening. In later life his habits were most abstemious, and it is said that his strength was in this way so reduced, as to render him unable to resist his last illness. After three days' suffering from nervous fever he succumbed on the 21st of August, when he was on the eve of returning to England. He was buried in the small cemetery of Auteuil, which has since been disused as a place of burial. The grave, says Dr. Ellis, is marked by a horizontal stone—une pierre tumulaire—and by a perpendicular monument 6 feet high, 6 feet in breadth, and $3\frac{1}{2}$ feet in thickness. Both are of marble and bear inscriptions as follows. That on the monument is:—

À la Mémoire
de
Benjamin Thompson,
Comte de Rumford,
né en 1753, à Concord * près Boston,
en Amérique,
mort le 21 Aout, 1814, à Auteuil.
Physicien célébré,
Philanthrope éclairé,
ses découvertes sur la lumière
et la chaleur
ont illustré son nom.

Ses travaux pour améliorer
le sort des pauvres
le feront toujours chéri
des amis de l'humanité.

---

* Woburn.

The flat stone is thus inscribed:—

En Bavière
Lieutenant-Général
Chef de l'Etat-Major Général,
Conseiller d'Etat,
Ministre de la Guerre.
En France
Membre de l'Institut,
Académie des Sciences.

Rumford's Scientific Work.

As a factor in human affairs, Rumford ascribed to gunpowder a dominant position. No other invention had exercised so great an influence. Hence the arduous labour he expended in determining its action. At Stoneland Lodge, the country seat of Lord George Germain, in the year 1778, his inquiries into the force and applications of gunpowder began. He directed his attention to the position of the vent, the weight and pressure of the charge, its bursting power, the quickness of combustion, the weight and velocity of the projectile, the effect of windage, and to many other matters of interest to the gunner. On all these questions he threw important light. The velocity was determined in two ways: first, by the ballistic pendulum, invented by his predecessor and namesake Benjamin Robins; and secondly from the recoil of the gun itself. The ballistic pendulum is a heavy mass, so suspended as to be capable of free oscillation. Against it the bullet is projected, and from the weight of the bullet, the weight of the pendulum, and the arc, or distance, through which it is urged by the bullet, the velocity of the latter may be calculated.

To determine the recoil of the gun, he had it suspended by a bifilar arrangement, which permitted it to swing back when it was fired. Action and reaction being equal, the momentum of the gun was the momentum of the bullet on leaving the gun, and from the weight of the piece, and the arc of recoil, the velocity of the bullet was computed. The agreement between the results obtained by these two methods was in many cases remarkable. Until quite recently, Rumford's experiments on the force of gunpowder were considered to be the best extant. A mind so observant could not fail to notice the heating effects produced by the percussion of the bullet against its target, and by the jar of the gun at the moment of its discharge. By such facts he was naturally led to reflect on that connection between mechanical power and the generation of heat which he afterwards did so much to illustrate and develope.

The phenomena both of light and heat fascinated him; and we accordingly find him from time to time abandoning practical aims, and seeking for knowledge which had no apparent practical outcome. Thus we see him experimenting on the action of green vegetables and

other matters upon light, or rather the action of light on the green leaves of plants. From this inquiry he turned to estimate the quantities of moisture taken up by different substances in humid air. Sheeps' wool he found to be the most absorbent, while cotton wool and ravellings of fine linen were among the least. These experiments he regarded as of the highest importance, as they explained, to his mind, the salubrity of flannel when worn next the skin. Its healthfulness he ascribed to its power of taking up the moisture of the body, sensible and insensible, and dispersing it by evaporation in the air.

The propagation of heat in fluids was but imperfectly understood when Rumford took the subject up. In various parts of his writings, he dwells on the importance of what he calls accidental observations, deeming them more fruitful than those which have sprung from the more recondite thoughts of the philosopher. But accidents, however numerous, if they fail to reach the proper soil are barren. Rumford ascribed to accident the investigations now referred to. He had been experimenting upon liquids, employing bulbs of copper with glass tubes attached to them. On one occasion, having filled his bulb and tube with spirits of wine, and heated the liquid, he placed it to cool in a window where the sun happened to shine upon it. Particles of dust had found their way into the spirit, and the sun, shining on these particles, made their motions vividly apparent. Along the axis of his tube the illuminated particles rose; along its sides they fell, thus making manifest the currents within the liquid. The reason of this circulation is obvious enough. The glass tube in contact with the cold air had its temperature lowered. The glass drew heat from the liquid in contact with it, which thereby being rendered more dense, fell along the sides of the tube, while, to supply its place, the lighter liquid rose along the axis. The motion here described is exactly that of the great geyser of Iceland. The water falls along the sides of the geyser tube, and rises along the axis. In this way then heat is propagated through liquids. It is a case of bodily transport by currents, and not one of true conduction from molecule to molecule.

It immediately occurred to Rumford to hamper this motion of convection. He called to mind an observation he had made at Baiæ, when the water of the sea being cool to the touch, the sand a few inches below the water was intolerably hot. This he ascribed to the impediment offered by the sand to the upward diffusion of the heat. The length of time required by stewed apples to cool, also occurred to him. He had frequently burnt his mouth by a spoonful of apple taken from the centre of a dish after the surface had become cool. He devised thermometers with a view of bringing his notions to an experimental test. With pure water he compared water slightly thickened with starch, water containing eider-down, and stewed apples bruised into a pulp which consisted almost wholly of water. In all cases he found the propagation of heat impeded, and cooling retarded, by everything that prevented the formation of currents. As he pursued his inquiries, the idea became more and more fixed in his mind that convection is the *only* means by which heat is diffused in liquids.

He denied them all power of true conduction, and though his experiments did not, and could not, prove this, they did prove that in the propagation of heat through the liquids he examined, which were water, oil, and mercury, conduction played an extremely subordinate part.

Rumford changes from time to time the tone of the philosopher for that of the preacher. He seems filled with religious enthusiasm on contemplating what he holds to be the wisdom and benevolence displayed in the arrangement of the physical world. One fact in particular excited this emotion. De Luc had pointed out that when water is cooled, it shrinks in volume, until it reaches a temperature of about 40° Fahr. At this point it ceases to contract, and expands when cooled still further. The expansion we now know to be due to incipient crystallisation, or freezing, which when it once sets in greatly enhances the expansion. A consequence of this is that ice floats as a lighter body upon water. This fact riveted the attention of Rumford, and its obvious consequences filled him with the enthusiasm to which I have referred. He was strong, but untrained, and his language was not always such as a truly disciplined man of science would employ. "Let me," he says, "beg the attention of my reader, while I endeavour to investigate this most interesting subject, and let me at the same time bespeak his candour and indulgency. I feel the danger to which a mortal exposes himself who has the temerity to undertake to explain the designs of Infinite Wisdom. The enterprise is adventurous, but it cannot surely be improper."

He "explains" accordingly; and, notwithstanding his professed humility, does not hesitate to brand those who fail to see with his eyes as "degraded, and quite callous to every ingenuous and noble sentiment." He indulges in excursions of the imagination with a view of rendering clear the misfortunes that would accrue if the arrangement of the world had been different from what it is. "Had not Providence, in a manner which may be well considered as miraculous," stopped the contraction of water before it reached its freezing-point, and caused it to expand afterwards, a single winter would freeze every fresh-water lake within the polar circle to a vast depth, "and it is more than probable that the regions of eternal frost would have spread on every side from the poles, and, advancing towards the equator, would have extended its dreary and solitary reign over a great part of what are now the most fertile and most inhabited climates of the world!" He expands this thesis in various directions, the whole argument being based on the assumption that "all bodies are condensed by cold, without limitation, WATER ONLY EXCEPTED." Repeated disappointments in such matters have taught us caution. Legitimate grounds for wonder exist everywhere around us; but wonder must not be cultivated at the expense of truth. Brought to the proper test, the assumption on which Rumford built his striking teleological argument is found to be a mere quicksand. The fact that he adduces as unique is not an exception to a universal law. There are other substances, to which

his reasoning has not the remotest application, which, like water, expand before and during crystallisation. The conditions necessary to the life of our planet must have existed before life appeared; but whether those conditions had prospective reference to life, or whether its immanent energy did not seize upon conditions which grew into being without any reference to life, we do not know; and it would be mere arrogance at the present day to dogmatise upon the subject.

In the controversy whether heat was a form of matter or a form of motion, Rumford espoused the latter view. Now those who supposed heat to be matter naturally thought that it might be ponderable, and experiments favourable to this notion had been executed. Operating with a balance of extreme delicacy, Rumford took up this question, and treated it with great skill and caution. His conclusion from his experiments was that, if heat be a substance—a fluid *sui generis*—it must be something so infinitely rare, even in its condensed state, as to baffle all our attempts to discover its gravity. But "if the opinion which has been adopted by many of our ablest philosophers, that heat is an intestine vibratory motion of the constituent parts of bodies, should be well founded, it is clear that the weights of bodies can be in no wise affected by such motion." The weight of a bell, he urges in another place, is not affected by its sonorous vibration.

Early in the year 1803, he being then in Munich, Rumford broke ground in the domain of radiant heat. He prepared bright metallic vessels, filled them with hot water, placed them in a large and quiet room, and observed the time required to cool them down a certain number of degrees. Covering some of his vessels with Irish linen and leaving others bare, he found, to his surprise, that the covered vessels were more rapidly chilled than the naked ones. Comparing in the same room a thick glass bottle, filled with hot water, with a tin bottle of the same shape and size, he found that the water in the glass vessel cooled twice as rapidly as that in the tin one. When, moreover, he coated his metallic vessel with glue, the cooling process was hastened, as it had been by the linen. Applying a second, a third, and a fourth coating of glue, he found the chilling promoted; but beyond this he came to a point where the addition of any further coatings produced a retardation of the chilling. Painting some of his vessels black and some white, he found the times of cooling to be practically the same for both—a result which he seems to have afterwards forgotten. From these and other experiments of the same kind he drew the just conclusion that a hot body does not lose its heat by the mere communication of it to the air, but that a large proportion of the heat escapes in *rays*, the escape being facilitated by the substances with which his vessels were coated. The more rapid chilling of the glass bottle was due, in like manner, to the fact that glass possesses a greater radiative power than tin.

He next applies himself with energy, zeal, and tenacity, to prove that there are frigorific rays which act in all respects like calorific rays, and which enjoy an individuality quite as assured as that of the

latter. He pictures his frigorific rays as produced by vibrations of a special kind. In Pictet's celebrated experiment of conjugate mirrors, and in many other experiments, chilling by a cold body showed itself to be so exactly analogous to heating by a warm one, that Rumford never could shake from his mind the notion of rays of cold. The fall of the thermometer in one focus when a lump of ice was placed in the other, was in his view caused by a positive emission of cold rays from the ice, and not to its absorption of the heat radiated against it by the thermometer. These frigorific rays, he says, were suspected by Bacon. Their existence was actually established by the academicians of Florence, but these learned gentlemen were so "blinded by their prejudices respecting the nature of heat, that they did not believe the report of their own eyes."

Rumford indulges in various untenable speculations and erroneous notions regarding the part played by clothing, by the blackness of the negro's skin, and by the oiled surface of the Hottentot. We are, he contends, kept warm by our clothing, not so much by confining our heat as by keeping off the frigorific rays which tend to cool us. He reverts to the respective cases of a black and a white man, and describes an experiment which elucidates his views. He covered two of his vessels with goldbeater's skin, and painted one of them black with Indian ink, leaving the other of its natural white colour. Filling both vessels with hot water, he left them to cool in the air of a large, quiet room. The vessel covered with the black skin represented a negro, the other vessel a white man; and the result was that while the black required only 23½ minutes to cool, the white man required 28 minutes. The practical issue of the experiment is thus stated:— "All I will venture to say on the subject is, that were I called to inhabit a very hot country, nothing should prevent me from making the experiment of blackening my skin, or, at least, of wearing a black shirt, in the shade, and especially at night, in order to find out if, by those means, I could not contrive to make myself more comfortable."

There was at times a headstrong element, if I may use the term, in Rumford's scientific reasoning. He here overlooks the fact that in a former experiment he found scarcely an appreciable difference between white and black as regards their powers of cooling. He also forgets the possible influence of a second coating, which his former experiments had revealed. As regards the negro and the white man, Rumford's first experiment illustrated the case more correctly than any subsequent ones. There are, moreover, transparent substances which, used as varnishes, would not have impaired the whiteness of the goldbeater's skin, but which would have hastened the cooling quite as much as the Indian ink.

Those who are acquainted with Sir John Leslie's experiments on radiant heat will not fail to notice that he and Rumford travelled over common ground. With a view of setting this matter right Rumford wrote a paper entitled "Historical Review of Experiments on the Subject of Heat," in which he shows that his experiments

were not only talked about and executed before learned societies, but that they were in part published prior to the appearance of Leslie's celebrated work in 1804. Still the style of that work furnishes, I think, internal evidence of its perfectly independent character, while the extent and variety of Leslie's labours render it practically impossible that they could have been derived from anything that Rumford had previously done. The two philosophers had no personal knowledge of each other, and the credit to be awarded, where they deal with the same subject, belongs, I think, equally to both.

Rumford's experimental work was far smaller in quantity than that of Leslie, but in regard to theory he must be conceded the highest place. In theory Leslie was inconsistent and confused, while Rumford, judged by the circumstances of his time, was in the main clear and correct. The part played by the luminiferous ether in the phenomena of light had been revived and enforced by the powerful experiments of Dr. Thomas Young. The undulatory hypothesis was therefore at hand, and Rumford made able use of it. He has written a paper entitled "Reflections on Heat," in which he describes the views regarding its nature that were prevalent in his time. "Some," he says, "regard it as a *substance*, others as a *vibratory motion* of the particles of matter of which a body is composed." The heating of a body is, on the one hypothesis, due to the accumulation within it of *caloric*, while others hold the heating to be due to the acceleration of the vibratory motion. "On the hypothesis of vibratory motion, a body which has become cold is thought to have lost nothing except motion; on the other hypothesis, it is supposed to have lost some material substance." The loss of motion Rumford clearly apprehends to be due to its communication to "an eminently elastic fluid—an ether which fills all space throughout the universe." The theoretic notions thus expressed are, in point of clearness and correctness, far in advance of those entertained by Leslie.

As already mentioned, the fact of water changing its density at a temperature of 40° Fahr., powerfully affected the mind of Rumford. On this subject he made many experiments; and one of the minor applications of the knowledge thus derived may be here noted. In company with his friend Professor Pictet, of Geneva, he paid a visit to the Mer de Glace, and discovered in the ice a pit "perfectly cylindrical, about 7 inches in diameter, and more than 4 feet deep, quite full of water." He was informed by his guides that these pits are formed in summer, and gradually increase in depth during the warm weather. How can these pits deepen? Rumford answers thus:—The warm winds which in summer blow over the surface of the column of ice-cold water, communicate some small degree of heat to the fluid. The water at the surface being thus rendered specifically heavier, sinks to the bottom of the pit, to which the heat thus carried down is communicated, the depth of the pit being thereby increased. We have here a small specimen of Rumford's penetration, but it is a very interesting one. The sun's invisible rays, however, are probably

more influential than the action of the warm wind in producing the observed effect.

Various interesting experiments were made by Rumford on what is now known as "surface tension." From his experiments he inferred that the surface of a liquid—of water for example—is covered by a pellicle which can be caused to tremble throughout, by touching it with the point of a needle. He proposed to the geometricians of Paris to determine the shape of a drop resting on a horizontal surface, and restrained solely by the resistance of a pellicle exerting a given force on its surface. This pellicle he considers to be due to the adhesion of the particles of liquids to each other, and he makes various ingenious calculations to determine the size of a particle of heavy matter, of gold for instance, which would rest suspended in water because of its inability to force asunder the particles of the liquid. The diameter of a sphere of gold which would behave in this way he found to be $\frac{1}{283505}$ of an inch.

Even among scientific men, probably few are aware that Rumford experimented on the diffusion of liquids; a field of investigation in which Graham afterwards rendered himself so eminent. Into a glass cylinder, $1\frac{3}{4}$ inch in diameter and 8 inches high, he poured a layer of saturated aqueous solution of muriate of soda 3 inches thick, over this he carefully poured a layer of distilled water of the same thickness, he then let a drop of the oil of cloves fall into the vessel. This oil, being heavier than the pure water and lighter than the solution, rested as a sphere at the common boundary of the two liquids. A layer of olive oil four lines in thickness was then poured over the water, in order to shut off the air. The object of the experiment was to ascertain whether one liquid remained permanently superposed upon the other without any mixing. If this proved to be the case the position of the drop of oil would remain constant; but if the heavy mineral solution rose into the water overhead, the drop of oil, which Rumford called his "little sentinel," would warn him of the event by rising in the liquid. After twenty-four hours he entered the cellar in which the experiment was made, and found that the little ball of oil had risen three lines. For six days it continued to rise at the rate of about three lines a day. He afterwards experimented with other solutions, the result being "that the mixture went on continually, but very slowly, between the various aqueous solutions employed and the distilled water resting upon them." Rumford's experiments were probably prompted by his views on molecular physics. He would hardly have thought of the foregoing arrangement were not the intestine motions of the ultimate particles of bodies present to his mind. He is, moreover, quite aware of the importance of the result which he has here established. He says that the subject has often occupied his thoughts, and that he had at different times made "a considerable number of experiments with a view of throwing light into the profound darkness with which the subject is shrouded on every side."

His manifold industry was devoted in part to steam considered as

a vehicle for transporting heat; on the means of increasing the heat obtained in the combustion of fuel; on a new steam boiler, in which we have a forecast of the tubular boiler of George Stevenson. After some preliminary experiments on wood and charcoal, he definitely takes up the important investigation of the quantity of heat developed in combustion, and in the condensation of vapours. He describes the new calorimeter employed in the inquiry. It was a kind of worm, through which the heated air and products of combustion were led, and in which the heat was delivered up to cold water surrounding the worm.

He experimented upon white wax, spirit of wine, alcohol, sulphuric ether, naphtha, charcoal, wood, and inflammable gases. Whenever it was possible he aimed at quantitative results, and in the present instance he "estimated the calorific power of a body by the number of parts, by weight, of water, which one part, by weight, of the body would, on perfect combustion, raise one degree in temperature. Thus, 1 lb. of charcoal, in combining with $2\frac{2}{3}$ lbs. of oxygen, to form carbonic acid, evolves heat sufficient to raise the temperature of about 8000 lbs. of water 1° C. Similarly, 1 lb. of hydrogen, in combining with 8 lbs. of oxygen, to form water, generates an amount of heat sufficient to raise 34,000 lbs. of water 1° C. The calorific powers, therefore, of carbon and hydrogen are as 8 : 34. The refined researches of Favre and Silbermann entirely confirm these determinations of Rumford."—(Percy.) Following the experiments on combustion, we have others made to determine the quantity of heat set free by the condensation of various vapours, and the capacity of various liquids for heat. We have also an elaborate inquiry into the structure of wood, the specific gravity of its solid parts, the liquids and elastic fluids contained in it, the quantity of charcoal to be obtained from it, and the heat generated by the combustion of wood of different kinds.

But the main object of Rumford's life and the subject which chiefly interested him was the practical management of fire, and the economy of fuel. Eighty-seven pages of the second volume of his collected works are devoted to this subject. The whole of the third volume is devoted to it, while a large portion of the fourth and last volume is occupied with kindred questions. Some of those essays are rather tiresome to a reader of the present day, and Rumford had a suspicion that they might appear so to contemporary readers. "I believe," he says, "that I am sometimes too prolix for the taste of the age; but it should be remembered that the subjects I have undertaken to investigate are by no means indifferent to me; that I conceive them to be intimately connected with the comforts and enjoyments of mankind; and that a habit of revolving them in my mind, and reflecting on their extensive usefulness, has awakened my enthusiasm, and rendered it quite impossible for me to treat them with cold indifference."

For the most part, it is only when Rumford is self-conscious that this tedium appears. He wishes to excite his reader's interest, and

sometimes adopts means to this end which defeat themselves. Such is the case when he dwells with reiteration on the refined and exquisite pleasure which he derives from being of service to humanity. Some also would deem him tedious, though I deem him courageous, when he deals with the details of his schemes. He leaves no stone unturned in his effort to render himself clear. He is in many cases simply writing out a specification, to be followed in all particulars. He gives directions as to the manner in which a slice of hasty pudding is to be eaten. A small pit is to be dug in the centre of the cake, a piece of butter placed in the pit, while the removed bit is to be placed on the butter to aid in melting it. You then begin at the circumference of your pudding, and eat all round, dipping each piece in the butter before conveying it to the mouth. Such details were sure to provoke sarcasm, and they did provoke it. But amid the verbosity we have incessant flashes of practical wisdom and examples of intellectual force. When he ceases to think of the exquisite delight of his philanthropic labours—ceases to think of himself—and permits his own personality to be effaced by his subject, we see Rumford at his best; and his best was excellent. Suggestion follows suggestion, experiment succeeds experiment, until he has finally exhausted his subject, or is pulled up by inability to proceed further.

He tested quantitatively the relative intensities of various lights, constructing, while doing so, his well-known photometer. Placing two lights in front of a white screen, and at the same distance from it, and fixing an opaque rod between the lights and the screen, he obtained two shadows corresponding to the two lights. When the lights were equally intense, the shadows were equally dark, but when one of the lights was more powerful than the other the shadow corresponding to that other was rendered pale, b cause the light from the most intense source fell upon it. Removing the more intense light further from the screen, until a point was reached when the shadows appeared equal, Rumford obtained all the elements necessary for the computation of the relative intensities of the lights. He had only to apply the law of inverse square, which makes a double distance correspond to a fourfold intensity, a treble distance to a ninefold intensity, and so on. In connection with these experiments he dwells repeatedly upon a defect which harasses the official gas examiners of the present day, and that is the fluctuations of the candles used as standards of measurement. These photometric measurements are succeeded by a brief, but beautiful essay on "Coloured Shadows," which, in connection with another short essay on the "Harmony of Colours," strikingly illustrates Rumford's penetration and experimental skill. He produced two shadows, one from daylight, the other from candlelight. The daylight shadow being shone upon by the candle, was, as might be expected, yellow, because the candle sheds a yellow light. But the other shadow, instead of being colourless, was "the most beautiful blue that it was possible to imagine." He states clearly that the colour of one shadow is real, while that of the other is imagi-

nary. He finds it "impossible to produce *two shadows* at the same time from the same body, the one answering to a beam of daylight, and the other to the light of a candle or lamp, without these shadows being coloured, the one yellow, and the other *blue*." He obtained shadows from a light coloured by means of interposed glasses, and compared them with shadows obtained from uncoloured light. The shadows were always coloured when the lights differed from each other in whiteness, and the colours of the shadows were always such as, when added together, produced a pure white. The real colour in fact, evoked, or "called up," or summoned an imaginary complementary colour. Goethe probably derived the expression "geförderte Farben," which occurs so often in the "Farbenlehre" from the terminology of Rumford.

But the experiments and discussion on which the fame of Rumford mainly rest are described in an essay of twenty pages, which almost vanishes in comparison with the sum total of his published work. A cannon foundry had been built under his superintendence at Munich, where the heat developed during the boring of cannon powerfully attracted his attention. Upon this heat he made numerous tentative experiments which are described in the essay. With the view of determining its exact quantity, he cut a cylinder from the muzzle end of a gun not yet bored, partially hollowed out this cylinder, and fitted into it a borer which resembled a blunt chisel in shape. The borer being strongly pressed against the bottom of the cylinder, it was caused to rotate by horse-power. He surrounded his cylinder with a wooden box, filling the box with water which embraced the entire cylinder. Soon after the starting of the rotation, the water felt warm to the hand. In an hour it had risen to 107° in temperature. In two hours and twenty minutes it had risen to 200°, while in two hours and thirty minutes it actually boiled.

"Rumford carefully estimated the quantity of heat possessed by each portion of his apparatus at the conclusion of his experiment, and adding all together, found a total sufficient to raise 26·58 lbs. of ice-cold water to its boiling-point, or through 180° Fahr. By careful calculation he found this heat equal to that given out by the combustion of 2303·8 grains ($= 4\frac{8}{10}$ oz. troy) of wax. He then determined the 'celerity' with which the heat was generated, summing up thus: 'From the results of these computations it appears that the quantity of heat produced equably, or in a continuous stream, if I may use the expression, by the friction of the blunt steel borer against the bottom of the hollow metallic cylinder, was greater than that produced in the combustion of nine wax candles, each three-quarters of an inch in diameter, all burning together with clear bright flames.'

"'One horse,' he continues, 'would have been equal to the work performed, though two were actually employed. Heat may thus be produced merely by the strength of a horse, and, in a case of necessity, this heat might be used in cooking victuals. But no circumstances

could be imagined in which this method of procuring heat would be advantageous; for more heat might be obtained by using the fodder necessary for the support of a horse as fuel.' "

This is an extremely significant passage, intimating, as it does, that Rumford saw clearly that the force of animals was derived from the food, no creation of force taking place in the animal body.

"By meditating on the results of all these experiments we are naturally," he says, "brought to the great question which has so often been the subject of speculation among philosophers, namely, What is heat—is there any such thing as an igneous fluid? Is there anything that, with propriety, can be called caloric?"

"We have seen that a very considerable quantity of heat may be excited by the friction of two metallic surfaces, and given off in a constant stream or flux in all directions, without interruption or intermission, and without any signs of diminution or exhaustion. In reasoning on this subject, we must not forget that most remarkable circumstance, that the source of the heat generated by friction in these experiments appeared evidently to be inexhaustible. It is hardly necessary to add that anything which any insulated body or system of bodies can continue to furnish without limitation cannot possibly be a material substance; and it appears to me to be extremely difficult, if not quite impossible, to form any distinct idea of anything capable of being excited and communicated in those experiments, except it be Motion." *

* 'Heat a Mode of Motion,' Lecture II.

WEEKLY EVENING MEETING,

Friday, January 18, 1884.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Professor Tyndall, D.C.L. F.R.S. *M.R.I.*

On Rainbows.

The oldest historic reference to the rainbow is known to all: "I do set my bow in the cloud, and it shall be for a token of a covenant between me and the earth. . . . And the bow shall be in the cloud; and I shall look upon it, that I may remember the everlasting covenant between God and every living creature of all flesh that is upon the earth." To the sublime conceptions of the theologian succeeded the desire for exact knowledge characteristic of the man of science. Whatever its ultimate cause might have been, the proximate cause of the rainbow was physical, and the aim of science was to account for the bow on physical principles. Progress towards this consummation was very slow. Slowly the ancients mastered the principles of reflection. Still more slowly were the laws of refraction dug from the quarries in which nature had embedded them. I use this language, because the laws were incorporate in nature before they were discovered by man. Until the time of Alhazan, an Arabian mathematician, who lived at the beginning of the twelfth century, the views entertained regarding refraction were utterly vague and incorrect. After Alhazan came Roger Bacon and Vitellio,* who made and recorded many observations and measurements on the subject of refraction. To them succeeded Kepler, who, taking the results tabulated by his predecessors, applied his amazing industry to extract from them their meaning—that is to say, to discover the physical principles which lay at their root. In this attempt he was less successful than in his astronomical labours. In 1604, Kepler published his 'Supplement to Vitellio,' in which he virtually acknowledged his defeat, by enunciating an approximate rule, instead of an all-satisfying natural law. The discovery of such a law, which constitutes one of the chief corner-stones of optical science, was made by Willebrord Snell, about 1621.†

* Whewell ('History of the Inductive Sciences,' vol. i. p. 345) describes Vitellio as a Pole. His mother was a Pole; but Poggendorff ('Handwörterbuch d. exacten Wissenschaften') claims Vitellio himself as a German, born in Thüringen. "Vitellio" is described as a corruption of Witelo.

† Born at Leyden 1591; died 1626.

A ray of light may, for our purposes, be presented to the mind as a luminous straight line. Let such a ray be supposed to fall vertically upon a perfectly calm water surface. The incidence, as it is called, is then perpendicular, and the ray goes through the water without deviation to the right or left. In other words, the ray in the air and the ray in the water form one continuous straight line. But the least deviation from the perpendicular causes the ray to be broken, or "refracted," at the point of incidence. What, then, is the law of refraction discovered by Snell? It is this, that no matter how the angle of incidence, and with it the angle of refraction, may vary, the relative magnitude of two lines, dependent on these angles, and called their sines, remains, for the same medium, perfectly unchanged. Measure, in other words, for various angles, each of these two lines with a scale, and divide the length of the longer one by that of the shorter; then, however the lines individually vary in length, the quotient yielded by this division remains absolutely the same. It is, in fact, what is called the index of refraction of the medium.

Science is an organic growth, and accurate measurements give coherence to the scientific organism. Were it not for the antecedent discovery of the law of sines, founded as it was on exact measurements, the rainbow could not have been explained. Again and again, moreover, the angular distance of the rainbow from the sun had been determined and found constant. In this divine remembrancer there was no variableness. A line drawn from the sun to the rainbow, and another drawn from the rainbow to the observer's eye, always enclosed an angle of 41°. Whence this steadfastness of position—this inflexible adherence to a particular angle? Newton gave to De Dominis * the credit of the answer; but we really owe it to the genius of Descartes. He followed with his mind's eye the rays of light impinging on a raindrop. He saw them in part reflected from the outside surface of the drop. He saw them refracted on entering the drop, reflected from its back, and again refracted on their emergence. Descartes was acquainted with the law of Snell, and taking up his pen he calculated, by means of that law, the whole course of the rays. He proved that the vast majority of them escaped from the drop as *divergent* rays, and, on this account, soon became so enfeebled as to produce no sensible effect upon the eye of an observer. At one particular angle, however—namely, the angle 41° aforesaid—they emerged in a practically parallel sheaf. In their union was strength, for it was this particular sheaf which carried the light of the "primary" rainbow to the eye.

There is a certain form of emotion called intellectual pleasure, which may be excited by poetry, literature, nature, or art. But I

---

* Archbishop of Spalatro, and Primate of Dalmatia. Fled to England about 1616; became a Protestant, and was made Dean of Windsor. Returned to Italy and resumed his Catholicism; but was handed over to the Inquisition, and died in prison (Poggendorff's 'Biographical Dictionary').

doubt whether among the pleasures of the intellect there is any more pure and concentrated than that experienced by the scientific man when a difficulty which has challenged the human mind for ages melts before his eyes, and recrystallises as an illustration of natural law. This pleasure was doubtless experienced by Descartes when he succeeded in placing upon its true physical basis the most splendid meteor of our atmosphere. Descartes showed, moreover, that the "secondary bow" was produced when the rays of light underwent two reflections within the drop, and two refractions at the points of incidence and emergence.

It is said that Descartes behaved ungenerously to Snell—that, though acquainted with the unpublished papers of the learned Dutchman, he failed to acknowledge his indebtedness. On this I will not dwell, for I notice on the part of the public a tendency, at all events in some cases, to emphasise such shortcomings. The temporary weakness of a great man is often taken as a sample of his whole character. The spot upon the sun usurps the place of his "surpassing glory." This is not unfrequent, but it is nevertheless unfair.

Descartes proved that according to the principles of refraction, a circular band of light must appear in the heavens exactly where the rainbow is seen. But how are the colours of the bow to be accounted for? Here his penetrative mind came to the very verge of the solution, but the limits of knowledge at the time barred his further progress. He connected the colours of the rainbow with those produced by a prism; but then these latter needed explanation just as much as the colours of the bow itself. The solution, indeed, was not possible until the composite nature of white light had been demonstrated by Newton. Applying the law of Snell to the different colours of the spectrum, Newton proved that the primary bow must consist of a series of concentric circular bands, the largest of which is red, and the smallest violet; while in the secondary bow these colours must be reversed. The main secret of the rainbow, if I may use such language, was thus revealed.

I have said that each colour of the rainbow is carried to the eye by a sheaf of approximately parallel rays. But what determines this parallelism? Here our real difficulties begin, but they are to be surmounted by attention. Let us endeavour to follow the course of the solar rays before and after they impinge upon a spherical drop of water. Take first of all the ray that passes through the centre of the drop. This particular ray strikes the back of the drop as a perpendicular, its reflected portion returning along its own course. Take another ray close to this central one and parallel to it—for the sun's rays when they reach the earth are parallel. When this second ray enters the drop it is refracted; on reaching the back of the drop it is there reflected, being a second time refracted on its emergence from the drop. Here the incident and the emergent rays enclose a small angle with each other. Take again a third ray a little further from the central one than the last. The drop

will act upon it as it acted upon its neighbour, the incident and emergent rays inclosing in this instance a larger angle than before. As we retreat further from the central ray the enlargement of this angle continues up to a certain point, where it reaches a maximum, after which further retreat from the central ray diminishes the angle. Now, a maximum resembles the ridge of a hill, or a watershed, from which the land falls in a slope at each side. In the case before us the divergence of the rays when they quit the raindrop would be represented by the steepness of the slope. On the top of the watershed—that is to say, in the neighbourhood of our maximum—is a kind of summit level, where the slope for some distance almost disappears. But the disappearance of the slope indicates, in the case of our raindrop, the absence of divergence. Hence we find that at our maximum, and close to it, there issues from the drop a sheaf of rays which are nearly, if not quite, parallel to each other. These are the so-called "effective rays" of the rainbow.*

Let me here point to a series of measurements which will illustrate the gradual augmentation of the deflection just referred to until it reaches its maximum, and its gradual diminution at the other side of the maximum. The measures correspond to a series of angles of incidence which augment by steps of ten degrees.

i	*d*	*i*	*d*
10°	10°	60°	42° 28′
20°	19° 36′	70°	39° 48′
30°	28° 20′	80°	31° 4′
40°	35° 36′	90°	15°
50°	40° 40′		

The figures in the column *i* express these angles, while under *d* we have in each case the accompanying deviation, or the angle enclosed by the incident and emergent rays. It will be seen that as the angle *i* increases, the deviation also increases up to 42° 28′, after which, although the angle of incidence goes on augmenting, the deviation becomes less. The maximum 42° 28′ corresponds to an incidence of 60°, but in reality at this point we have already passed, by a small quantity, the exact maximum, which occurs between 58° and 59°. Its amount is 42° 30′. This deviation corresponds to the red band of the rainbow. In a precisely similar manner the other colours rise to their maximum, and fall on passing beyond it; the maximum

* There is, in fact, a bundle of rays near the maximum, which, when they enter the drop, are converged by refraction almost exactly to the same point at its back. If the convergence were *quite* exact, then the symmetry of the liquid sphere would cause the rays to quit the drop as they entered it—that is to say, perfectly parallel. But inasmuch as the convergence is not quite exact, the parallelism after emergence is only approximate. The emergent rays cut each other at extremely sharp angles, thus forming a "caustic" which has for its asymptote the ray of maximum deviation. In the secondary bow we have to deal with a minimum, instead of a maximum, the crossing of the incident and emergent rays producing the observed reversal of the colours. (See Engel and Shellbach's diagrams of the rainbow.)

for the violet band being 40° 30′. The entire width of the primary rainbow is therefore 2°, part of this width being due to the angular magnitude of the sun.

We have thus revealed to us the geometric construction of the rainbow. But though the step here taken by Descartes and Newton was a great one, it left the theory of the bow incomplete. Within the rainbow proper, in certain conditions of the atmosphere, are seen a series of richly-coloured zones, which were not explained by either Descartes or Newton. They are said to have been first described by Mariotte,* and they long challenged explanation. At this point our difficulties thicken, but, as before, they are to be overcome by attention. It belongs to the very essence of a maximum, approached continuously on both sides, that on the two sides of it pairs of equal value may be found. The maximum density of water, for example, is 39° Fahrenheit. Its density when 5° colder, and when 5° warmer, than this maximum is the same. So also with regard to the slopes of our watershed. A series of pairs of points of the same elevation can be found upon the two sides of the ridge; and, in the case of the rainbow, on the two sides of the maximum deviation we have a succession of pairs of rays having the same deflection. Such rays travel along the same line, and add their forces together after they quit the drop. But light, thus reinforced by the coalescence of non-divergent rays, ought to reach the eye. It does so; and were light what it was once supposed to be—a flight of minute particles sent by luminous bodies through space—then these pairs of equally deflected rays would diffuse brightness over a large portion of the area within the primary bow. But inasmuch as light consists of *waves* and not of particles, the principle of interference comes into play, in virtue of which waves can alternately reinforce and destroy each other. Were the distance passed over, by the two corresponding rays within the drop, the same, they would emerge as they entered. But in no case are the distances the same. The consequence is that when the rays emerge from the drop they are in a condition either to support or to destroy each other. By such alternate reinforcement and destruction, which occur at different places for different colours, the coloured zones are produced within the primary bow. They are called "supernumerary bows," and are seen, not only within the primary but sometimes also outside the secondary bow. The condition requisite for their production is, that the drops which constitute the shower shall all be of nearly the same size. When the drops are of different sizes, we have a confused superposition of the different colours, an approximation to white light being the consequence. This second step in the explanation of the rainbow was taken by a man the quality of whose genius resembled that of Descartes or Newton, and who eighty-two years ago was appointed Professor of Natural Philosophy in the Royal

* Prior of St. Martin-sous-Beaune, near Dijon. Member of the French Academy of Sciences. Died in Paris, May 1684.

Institution of Great Britain. I refer, of course, to the illustrious Thomas Young.*

But our task is not, even now, complete. The finishing touch of the explanation of the rainbow was given by our last, eminent, Astronomer Royal, Sir George Airy. Bringing the knowledge possessed by the founders of the undulatory theory, and that gained by subsequent workers, to bear upon the question, Sir George Airy showed that, though Young's general principles were unassailable, his calculations were sometimes wide of the mark. It was proved by Airy that the curve of maximum illumination in the rainbow does not quite coincide with the geometric curve of Descartes and Newton. He also extended our knowledge of the supernumerary bows, and corrected the positions which Young had assigned to them. Finally, Professor Miller, of Cambridge, and Dr. Galle, of Berlin, illustrated by careful measurements with the theodolite the agreement which exists between the theory of Airy and the facts of observation. Thus, from Descartes to Airy, the intellectual force expended in the elucidation of the rainbow, though broken up into distinct personalties, might be regarded as that of an individual artist, engaged throughout this time in lovingly contemplating, revising, and perfecting his work.

We have thus cleared the ground for the series of experiments which constitute the subject of this discourse. During our brief residence in the Alps this year, we were favoured with some weather of matchless perfection ; but we had also our share of foggy and drizzly weather. On the night of the 22nd of September, the atmosphere was especially dark and thick. At 9 P.M. I opened a door at the end of a passage and looked out into the gloom. Behind me hung a small lamp, by which the shadow of my body was cast upon the fog. Such a shadow I had often seen, but in the present case it was accompanied by an appearance which I had not previously seen. Swept through the darkness round the shadow, and far beyond, not only its boundary, but also beyond that of the illuminated fog, was a pale, white, luminous circle, complete except at the point where it was cut through by the shadow. As I walked out into the fog, this curious halo went in advance of me. Had not my demerits been so well known to me, I might have accepted the phenomenon as an evidence of canonisation. Benvenuto Cellini saw something of the kind surrounding his shadow, and ascrbied it forthwith to supernatural favour. I varied the position and intensity of the lamp, and found even a candle sufficient to render the luminous band visible. With two crossed laths I roughly measured the angle subtended by the radius of the circle, and found it to be practically the angle which had riveted the attention of Descartes—namely, 41°. This and other facts led me to suspect that the halo was a circular rainbow. A week

* Young's 'Works,' edited by Peacock, vol. i. pp. 185, 293, 357.

subsequently, the air being in a similar misty condition, the luminous circle was well seen from another door, the lamp which produced it standing on a table behind me.

It is not, however, necessary to go to the Alps to witness this singular phenomenon. Amid the heather of Hind Head I have had erected a hut, to which I escape when my brain needs rest or my muscles lack vigour. The hut has two doors, one opening to the north and the other to the south, and in it we have been able to occupy ourselves pleasantly and profitably during the recent misty weather. Removing the shade from a small petroleum lamp, and placing the lamp behind me, as I stood in either doorway, the luminous circles surrounding my shadow on different nights were very remarkable. Sometimes they were best to the north, and sometimes the reverse, the difference depending for the most part on the direction of the wind. On Christmas night the atmosphere was particularly good-natured. It was filled with true fog, through which, however, descended palpably an extremely fine rain. Both to the north and to the south of the hut the luminous circles were on this occasion specially bright and well-defined. They were, as I have said, swept through the fog far beyond its illuminated area, and it was the darkness against which they were projected which enabled them to shed so much apparent light. The "effective rays," therefore, which entered the eye in this observation gave *direction*, but not distance, so that the circles appeared to come from a portion of the atmosphere which had nothing to do with their production. When the lamp was taken out into the fog, the illumination of the medium almost obliterated the halo. Once educated, the eye could trace it, but it was toned down almost to vanishing. There is some advantage, therefore, in possessing a hut, on a moor or on a mountain, having doors which limit the area of fog illuminated.

I have now to refer to another phenomenon which is but rarely seen, and which I had an opportunity of witnessing on Christmas Day. The mist and drizzle in the early morning had been very dense; a walk before breakfast caused my somewhat fluffy pilot dress to be covered with minute water-globules, which, against the dark background underneath, suggested the bloom of a plum. As the day advanced, the south-eastern heaven became more luminous; and the pale disk of the sun was at length seen struggling through drifting clouds. At ten o'clock the sun had become fairly victorious, the heather was adorned by pendent drops, while certain branching grasses, laden with liquid pearls, presented, in the sunlight, an appearance of exquisite beauty. Walking across the common to the Portsmouth road, my wife and I, on reaching it, turned our faces sunwards. The smoke-like fog had vanished, but its disappearance was accompanied, or perhaps caused, by the coalescence of its minuter particles into little globules, visible where they caught the light at a proper angle, but not otherwise. They followed every eddy of the air, upwards, downwards, and from side to side. Their extreme mobility was well

calculated to suggest a notion prevalent on the Continent, that the particles of a fog, instead of being full droplets, are really little bladders or vesicles. Clouds are supposed to owe their power of flotation to this cause. This vesicular theory never struck root in England; nor has it, I apprehend, any foundation in fact.

As I stood in the midst of these eddying specks, so visible to the eye, yet so small and light as to be perfectly impalpable to the skin both of hands and face, I remarked, "These particles must surely yield a bow of some kind." Turning my back to the sun, I stooped down so as to keep well within the layer of particles, which I supposed to be a shallow one, and, looking towards the "Devil's Punch Bowl," saw the anticipated phenomenon. A bow without colour spanned the Punch Bowl, and, though white and pale, was well defined, and exhibited an aspect of weird grandeur. Once or twice I fancied a faint ruddiness could be discerned on its outer boundary. The stooping was not necessary, and as we walked along the new Portsmouth road, with the Punch Bowl to our left, the white arch marched along with us. At a certain point we ascended to the old Portsmouth road, whence with a flat space of very dark heather in the foreground, we watched the bow. The sun had then become strong, and the sky above us blue, nothing which could in any proper sense be called rain existing at the time in the atmosphere. Suddenly my companion exclaimed, "I see the whole circle meeting at my feet!" At the same moment the circle became visible to me also. It was the darkness of our immediate foreground that enabled us to see the lower half of the pale luminous band projected against it. We walked round Hind Head Common with the bow almost always in view. Its crown sometimes disappeared, showing that the minute globules which produced it did not extend to any great height in the atmosphere. In such cases, two shining buttresses were left behind, which, had not the bow been previously seen, would have lacked all significance. In some of the combes, or valleys, where the floating particles had collected in greater numbers, the end of the bow plunging into the combe emitted a light of more than the usual brightness. During our walk, the bow was broken and re-formed several times; and, had it not been for our previous experience, both in the Alps and at Hind Head, it might well have escaped attention. What this white bow lost in beauty and intensity, as compared with the ordinary coloured bow, was more than atoned for by its weirdness and its novelty to both observers.

The white rainbow (*l'arc-en-ciel blanc*) was first described by the Spaniard Don Antonio de Ulloa, Lieutenant of the Company of Gentleman Guards of the Marine. By order of the King of Spain, Don Jorge Juan and Ulloa made an expedition to South America, an account of which is given in two amply-illustrated quarto volumes to be found in the library of the Royal Institution. The bow was observed from the summit of the mountain Pambamarca, in Peru. The angle subtended by its radius was 33° 30′, which is considerably less than the angle subtended by the radius of the ordinary bow.

Between the phenomenon observed by us on Christmas Day, and that described by Ulloa, there are some points of difference. In his case fog of sufficient density existed to enable the shadows of him and his six companions to be seen, each, however, only by the person whose body cast the shadow, while around the head of each were observed those zones of colour which characterise the "spectre of the Brocken." In our case no shadows were to be seen, for there was no fog-screen on which they could be cast. This implies also the absence of the zones of colour observed by Ulloa.

The white rainbow has been explained in various ways. A learned Frenchman, M. Bravais, who has written much on the optical phenomena of the atmosphere, and who can claim the additional recommendation of being a distinguished mountaineer, has sought to connect the bow with the vesicular theory to which I have just referred. This theory, however, is more than doubtful, and it is not necessary.* The genius of Thomas Young throws light upon this subject as upon so many others. He showed that the whiteness of the bow was a direct consequence of the smallness of the drops which produce it. In fact, the wafted water-specks seen by us upon Hind Head † were the very kind needed for the production of the phenomenon. But the observations of Ulloa place his white bow distinctly *within* the arc that would be occupied by the ordinary rainbow—that is to say, in the region of supernumeraries; and by the action of the supernumeraries upon each other Ulloa's bow was accounted for by Thomas Young. The smaller the drops the broader are the zones of the supernumerary bows, and Young proved by calculation that when the drops have a diameter of $\frac{1}{3000}$th or $\frac{1}{4000}$th of an inch, the bands overlap each other, and produce white light by their mixture. Unlike the geometric bow, the radius of the white bow varies within certain limits, which M. Bravais shows to be 33° 30′ and 41° 46′ respectively. In the latter case the white bow is the ordinary bow deprived of its colour by the smallness of the drops. In all the other cases it is produced by the action of the supernumeraries.

The physical investigator desires not only to observe natural phenomena but to re-create them—to bring them, that is, under the dominion of experiment. From observation we learn what nature is willing to reveal. In experimenting we place her in the witness-box, cross-examine her, and extract from her knowledge in excess of that which would, or could, be spontaneously given. Accordingly, on my return from Switzerland last October, I sought to reproduce in the laboratory the effects observed among the mountains. My first object,

---

* The vesicular theory was combated very ably in France by the Abbé Raillard, who has also given an interesting analysis of the rainbow at the end of his translation of my 'Notes on Light.'

† Had our refuge in the Alps been built on the southern side of the valley of the Rhone, so as to enable us to look with the sun behind us into the valley and across it, we should, I think, have frequently seen the white bow; whereas on the opposite mountain slope, which faces the sun, we have never seen it.

therefore, was to obtain artificially a mixture of fog and drizzle like that observed from the door of our cottage. A strong cylindrical copper boiler, 16 inches high, and 12 inches in diameter, was nearly filled with water, and heated by gas flames until steam of twenty pounds pressure was produced. A valve at the top of the boiler was then opened, when the steam issued violently into the atmosphere, carrying droplets of water mechanically along with it, and condensing above to droplets of a similar kind. A fair imitation of the Alpine atmosphere was thus produced. After a few tentative experiments, the luminous circle was brought into view, and having once got hold of it, the next step was to enhance its intensity. Oil lamps, the lime-light, and the naked electric light were tried in succession, the source of rays being placed in one room, the boiler in another, while the observer stood, with his back to the light, between them. It is not, however, necessary to dwell upon these first experiments, surpassed as they were by the arrangements subsequently adopted. My mode of proceeding was this. The electric light being placed in a camera with a condensing lens in front, the position of the lens was so fixed as to produce a beam sufficiently broad to clasp the whole of my head, and leave an aureole of light around it. It being desirable to lessen as much as possible the foreign light entering the eye, the beam was received upon a distant black surface, and it was easy to move the head until its shadow occupied the centre of the illuminated area. To secure the best effect it was found necessary to stand close to the boiler, so as to be immersed in the fog and drizzle. The fog, however, was soon discovered to be a mere nuisance. Instead of enhancing, it blurred the effect, and I therefore sought to abolish it. Allowing the steam to issue for a few seconds from the boiler, on closing the valve, the cloud rapidly melted away, leaving behind it a host of minute liquid spherules floating in the beam. A beautiful circular rainbow was instantly swept through the air in front of the observer. The primary bow was duly attended by its secondary, with the colours, as usual, reversed. The opening of the valve for a single second causes the bows to flash forth. Thus, twenty times in succession, puffs can be allowed to issue from the boiler, every puff being followed by this beautiful meteor. The bows produced by single puffs are evanescent, because the little globules rapidly disappear. Greater permanence is secured when the valve is left open for an interval sufficient to discharge a copious amount of drizzle into the air.*

* It is perhaps worth noting here, that when the camera and lens are used, the beam which sends its "effective rays" to the eye may not be more than a foot in width, while the circular bow engendered by these rays may be, to all appearance, fifteen or twenty feet in diameter. In such a beam, indeed, the drops which produce the bow must be very near the eye, for rays from the more distant drops would not attain the required angle. The apparent distance of the circular bow is often great in comparison with that of the originating drops. Both distance and diameter may be made to undergo variations. In the rainbow we do not see a localised object, but receive a luminous impression, which is often transferred to a portion of the field of view far removed from the bow's origin.

Many other appliances for producing a fine rain have been tried, but a reference to two of them will suffice. The rose of a watering-pot naturally suggests a means of producing a shower; and on the principle of the rose I had some spray-producers constructed. In each case the outer surface was convex, the thin convex metal plate being pierced by orifices too small to be seen by the naked eye. Small as they are, fillets of very sensible magnitude issue from the orifices, but at some distance below the spray-producer the fillets shake themselves asunder and form a fine rain. The small orifices are very liable to get clogged by the particles suspended in London water. In experiments with the rose, filtered water was therefore resorted to. A large vessel was mounted on the roof of the Royal Institution, from the bottom of which descended vertically a piece of compo-tubing, an inch in diameter and about twenty feet long. By means of proper screw fittings, a single rose, or, when it is desired to increase the magnitude or density of the shower, a group of two, three, or four roses, is attached to the end of the compo-tube. From these, on the turning on of a cock, the rain descends. The circular bows produced by such rain are far richer in colour than those produced by the smaller globules of the condensed steam. To see the effect in all its beauty and completeness, it is necessary to stand well within the shower, not outside of it. A waterproof coat and cap are therefore needed, to which a pair of goloshes may be added with advantage. A person standing outside the beam may see bits of both primary and secondary in the places fixed by their respective angles; but the colours are washy and unimpressive, while within the shower, with the shadow of the head occupying its proper position on the screen, the brilliancy of the effect is extraordinary. The primary clothes itself in the richest tints, while the secondary, though less vivid, shows its colours in surprising strength and purity.

But the primary bow is accompanied by appearances calculated to attract and rivet attention almost more than the bow itself. I have already mentioned the existence of effective rays over and above those which go to form the geometric bow. They fall within the primary, and, to use the words of Thomas Young, "would exhibit a continued diffusion of fainter light, but for the general law of interference which divides the light into concentric rings." One could almost wish for the opportunity of showing Young how literally his words are fulfilled and how beautifully his theory is illustrated, by these artificial circular rainbows. For here the space within the primaries is swept by concentric supernumerary bands, coloured like the rainbow, and growing gradually narrower as they retreat from the primary. These spurious bows, as they are sometimes called,* which constitute one of the most splendid illustrations of the principle of interference, are separated from each other by zones of darkness, where the light waves, on being added together, destroy each other.

* A term, I confess, not to my liking.

I have counted as many as eight of these beautiful bands, concentric with the true primary. The supernumeraries are formed next to the most refrangible colour of the bow, and therefore occur *within* the primary circle. But in the secondary bow, the violet, or most refrangible colour, is on the *outside*; and, following the violet of the secondary, I have sometimes counted as many as five spurious bows. Some notion may be formed of the intensity of the primary, when the secondary is able to produce effects of this description.

An extremely handy spray-producer is that employed to moisten the air in the Houses of Parliament. A fillet of water, issuing under strong pressure from a small orifice, impinges on a little disk, placed at a distance of about one-twentieth of an inch from the orifice. On striking the disk, the water spreads laterally, and breaks up into exceedingly fine spray. Here also I have used the spray-producer both singly and in groups, the latter arrangement being resorted to when showers of special breadth and density were required. In regard to primaries, secondaries, and supernumeraries, extremely brilliant effects have been obtained with this form of spray-producer. The quantity of water called upon being much less than that required by the rose, the fillet-and-disk instrument produces less flooding of the locality where the experiments are made. In this latter respect, the steam spray is particularly handy. A puff of two seconds' duration suffices to bring out the bows, the subsequent shower being so light as to render the use of waterproof clothing unnecessary. In other cases, the inconvenience of flooding may be avoided to a great extent by turning on the spray for a short time only, and then cutting off the supply of water. The vision of the bow being, however, proportionate to the duration of the shower, will, when the shower is brief, be evanescent. Hence, when quiet and continued contemplation of all the phenomena is desired, the observer must make up his mind to brave the rain.*

In one important particular the spray-producer last described commends itself to our attention. With it we can operate on substances more costly than water, and obtain rainbows from liquids of the most various refractive indices. To extend the field of experiment in this direction, the following arrangement has been devised: A strong cylindrical iron bottle, wholly or partly filled with the liquid to be experimented on, is tightly closed by a brass cap. Through the cap passes a metal tube, soldered air-tight where it crosses the cap, and ending near the bottom of the iron bottle. To the free end of this tube is attached the spray-producer. A second tube passes also through the cap, but ends above the surface of the liquid. This second tube, which is long and flexible, is connected with a larger iron bottle, containing compressed air. Hoisting the small bottle to a convenient height, the tap of the larger bottle is carefully opened, the air passes through the flexible tube to the smaller bottle, exerts

* The rays which form the artificial bow emerge, as might be expected, polarised from the drops.

its pressure upon the surface of the liquid therein contained, drives it up the other tube, and causes it to impinge with any required degree of force against the disk of the spray-producer. From this it falls in a fine rain. A great many liquids, including coloured ones,* have been tested by this arrangement, and very remarkable results have been obtained. I will confine myself here to a reference to two liquids, which commend themselves on account of their cheapness and of the brilliancy of their effects. Spirit of turpentine, forced from the iron bottle, and caused to fall in a fine shower, produces a circular bow of extraordinary intensity and depth of colour. With paraffin oil or petroleum a similar effect is obtained.

Spectrum analysis, as generally understood, occupies itself with atomic, or molecular, action, but physical spectrum analysis may be brought to bear upon our falling showers. I asked myself whether a composite shower—that is to say, one produced by the mingled spray of two or more liquids—could not be analysed and made to declare its constituents by the production of the circular rainbows proper to the respective liquids. This was found to be the case. In the ordinary rainbow the narrowest colour-band is produced by its most refrangible light. In general, the greater the refraction, the smaller will be the bow. Now, as spirit of turpentine and paraffin are both more refractive than water, I thought it probable that in a mixed shower of water and paraffin, or water and turpentine, the smaller and more luminous circle of the latter ought to be seen within the larger circle of the former. The result was exactly in accordance with this anticipation. Beginning with water, and producing its two bows, and then allowing the turpentine to shower down and mingle with the water, within the large and beautifully coloured water-wheel, the more richly coloured circle of the turpentine makes its appearance. Or, beginning with turpentine, and forming its concentrated iris; on turning on the water-spray, though to the eye the shower seems absolutely homogeneous, its true character is instantly declared by the flashing out of the larger concentric aqueous bow. The water primary is accompanied by its secondary close at hand. Associated, moreover, with all the bows, primary and secondary, are the supernumeraries which belong to them; and a more superb experimental illustration of optical principles it would be hardly possible to witness. It is not the less impressive because extracted from the simple combination of a beam of light and a shower of rain.

In the 'Philosophical Transactions' for 1835, the late Colonel Sykes gave a vivid description of a circular solar rainbow, observed by him in India, during periods when fogs and mists were prevalent in the chasms of the Ghâts of the Deccan.

"It was during such periods that I had several opportunities of witnessing that singular phenomenon, the circular rainbow, which,

* Rose-aniline, dissolved in alcohol, produces a splendid bow with specially broad supernumeraries.

from its rareness, is spoken of as a possible occurrence only. The stratum of fog from the Konkun on some occasions rose somewhat above the level of the top of a precipice forming the north-west scarp of the hill fort of Hurreechundurghur, from 2000 to 3000 feet perpendicular, without coming over upon the table-land. I was placed at the edge of the precipice just without the limits of the fog, and with a cloudless sun at my back at a very low elevation. Under such a combination of favourable circumstances, the circular rainbow appeared quite perfect, of the most vivid colours, one half above the level on which I stood, the other half below it. Shadows in distinct outline of myself, my horse, and people appeared in the centre of the circle as a picture, to which the bow formed a resplendent frame. My attendants were incredulous that the figures they saw under such extraordinary circumstances could be their own shadows, and they tossed their arms and legs about, and put their bodies into various postures, to be assured of the fact by the corresponding movements of the objects within the circle; and it was some little time ere the superstitious feeling with which the spectacle was viewed wore off. From our proximity to the fog, I believe the diameter of the circle at no time exceeded fifty or sixty feet. The brilliant circle was accompanied by the usual outer bow in fainter colours."

Mr. E. Colborne Baber, an accomplished and intrepid traveller, has recently enriched the 'Transactions' of the Royal Geographical Society by a paper of rare merit, in which his travels in Western China are described. He made there the ascent of Mount O—an eminence of great celebrity. Its height is about 11,000 feet above the sea, and it is flanked on one side by a cliff "a good deal more than a mile in height." From the edge of this cliff, which is guarded by posts and chains, you look into an abyss, and if fortune, or rather the mists, favour you, you see there a miracle, which is thus described by Mr. Baber:—

"Naturally enough it is with some trepidation that pilgrims approach this fearsome brink, but they are drawn to it by the hope of beholding the mysterious apparition known as the 'Fo-Kuang,' or 'Glory of Buddha,' which floats in mid-air, half-way down. So many eye-witnesses had told me of this wonder, that I could not doubt; but I gazed long and steadfastly into the gulf without success, and came away disappointed, but not incredulous. It was described to me as a circle of brilliant and many-coloured radiance, broken on the outside with quick flashes and surrounding a central disk as bright as the sun, but more beautiful. Devout Buddhists assert that it is an emanation from the aureole of Buddha, and a visible sign of the holiness of Mount O.

"Impossible as it may be deemed, the phenomenon does really exist. I suppose no better evidence could be desired for the attestation of a Buddhist miracle than that of a Baptist missionary, unless, indeed, it be, as in this case, that of *two* Baptist missionaries. Two gentlemen of that persuasion have ascended the mountain since my visit, and

have seen the Glory of Buddha several times. They relate that it resembles a golden sun-like disk, enclosed in a ring of prismatic colours more closely blended than in the rainbow. . . . The missionaries inform me that it was about three o'clock in the afternoon, near the middle of August, when they saw the meteor, and that it was only visible when the precipice was more or less clothed in mist. It appeared to lie on the surface of the mist, and was always in the direction of a line drawn from the sun through their heads, as is certified by the fact that the shadow of their heads was seen on the meteor. They could get their heads out of the way, so to speak, by stooping down, but are not sure if they could do so by stepping aside. Each spectator, however, could see the shadows of the bystanders as well as his own projected on to the appearance. They did not observe any rays spreading from it. The central disk, they think, is a reflected image of the sun, and the inclosing ring is a rainbow. The ring was in thickness about one-fourth of the diameter of the disk, and distant from it by about the same extent; but the recollection of one informant was that the ring touched the disk, without any intervening space. The shadow of a head, when thrown upon it, covered about one-eighth of the whole diameter of the meteor. The rainbow ring was not quite complete in its lower part, but they attribute this to the interposition of the edge of the precipice. They see no reason why the appearance should not be visible at night when the moon is brilliant and appositely placed. They profess themselves to have been a good deal surprised, but not startled, by the spectacle. They would consider it remarkable rather than astonishing, and are disposed to call it a very impressive phenomenon."

It is to be regretted that Mr. Baber failed to see the "Glory," and that we in consequence miss his own description of it. There seems a slight inadvertence in the statement that the head could be got out of the way by stooping; for, as long as the "Glory" remained a circle, the shadow of the head must have occupied its centre. Stepping aside would simply displace the bow, but not abolish the shadow.

Thus, starting from the first faint circle seen drawn through the thick darkness at Alp Lusgen, we have steadily followed and developed our phenomenon, and ended by rendering the "Glory of Buddha" a captive of the laboratory. The result might be taken as typical of larger things.

[J. T.]

WEEKLY EVENING MEETING,

Friday, January 25, 1884.

Warren De La Rue, Esq. M.A. D.C.L. F.R.S. Vice-President, in the Chair.

H. H. Johnston, Esq.

Kilima-njâro, the Snowclad Mountain of Equatorial Africa.

(No Abstract received.)

WEEKLY EVENING MEETING,

Friday, February 1, 1884.

George Busk, Esq. F.R.S. Treasurer and Vice President, in the Chair.

Professor F. Max Müller.

Râjah Râmmohun Roy, the Religious Reformer of India.

Professor Max Müller began by reminding his audience that he had in former years often spoken to them about the Veda, the sacred book of the Brâhmans. Many might have wondered why he should have devoted the whole or at least the best part of his life to the publication of the text and commentary of this one book; but he still felt convinced that there was no book more ancient and more important in the whole literature of the Aryan race, and that it would continue to occupy the attention of scholars and philosophers as long as men cared for their own history and for the early development of language, thought, and religion. He wished, however, to show that the Veda was not only the most ancient, but, in one sense also, the most modern of books; that, directly or indirectly, it still was the foundation of the religion of 163,000,000 of human beings, for whom we ought to feel the deepest sympathy. He adverted to the statements of certain scholars and Indian tourists that the Veda was dead and forgotten, and that the real religion of the Hindus was nothing but the most hideous idolatry.

After showing how difficult it is for travellers ignorant of the ancient language and literature of India to understand what they see in that country, the lecturer proceeded to define what was really meant by the religion of a country, and how it ought to be studied. He quoted several misrepresentations of the true character of the Hindus, among others, the statement lately made by a gentleman long resident in India, that a Hindu would rather kill a woman than a cow, a statement which was about as true as that an Englishman would rather shoot his wife than a fox. Instead, however, of dealing in assertions and counter-assertions, he should prefer to put the question as to the importance of the Veda even in modern times to a practical test, and he proceeded to do so by examining the life and works of the greatest religious reformer of modern India, Râmmohun Roy.

In India, he said, there is no taste for history, still less for biography. Home life and family life are shrouded by a veil which no one ventures to lift, while public life has as yet hardly any existence in the East. What we know, therefore, of the external life of Râmmohun Roy is very little, and even that little often very doubtful. Râmmohun Roy was born in 1774. His ancestors on the paternal and maternal sides belonged to the Brâhmanic aristocracy. He himself was educated for secular life, as his father had been before him,

and he devoted most of his time as a boy to a study of Persian and Arabic rather than of Sanskrit. Through his knowledge of the Koran he was led from his early youth to entertain the strongest aversion to idolatry and polytheism, and his outspoken contempt for his family idols led to serious misunderstandings between him and his parents. The story that at the age of sixteen he wrote a book "Against the idolatry of all religions" seems to rest on no good authority. But there can be no doubt that at that early age Râmmohun Roy had to leave his paternal home to travel for many years in India and even beyond the frontiers of India.

After his return he entered the service of the East India Company, and began to acquire a knowledge of English, which was at that time a very rare acquirement for a Hindu gentleman. His hatred of everything English was changed into a feeling of sincere respect through his acquaintance with members of the Civil Service, and he was filled with admiration for some of the master-works of English literature. After he had acquired a sufficient fortune, partly by his own exertions, partly by inheritance, he bought a house at Calcutta and settled there in 1814. His house soon became the centre of the more enlightened native society of Calcutta. The relations between Englishmen and natives were far more cordial at that time than they are now, and even such subjects as religion and native customs were freely discussed between them. In these discussions Râmmohun Roy maintained that idolatry and polytheism were mere corruptions of the ancient religion of India, and that the only book in which that ancient religion could be studied was the Veda. He boldly denounced the malpractices of the priests, published extracts from the Veda to show that their teaching contravened the letter and spirit of their own Bible, and thus gathered around him a number of followers who tried to bring the religion of the people back to its original purity and simplicity. He opposed the burning of widows, and at last succeeded in having that hideous custom put down by law.

He then turned to the study of the Old and the New Testaments, and learnt sufficient Hebrew and Greek to be able to read these books in the original. He afterwards published the 'Precepts of Jesus, the Guide to Peace and Happiness,' and wrote in the preface:— "This simple code of religion and morality is so admirably calculated to elevate man's ideas to high and liberal notions of one God, who has originally subjected all living creatures, without distinction of caste, rank, or wealth, to change, disappointment, pain, and death, and has equally admitted all to be partakers of the bountiful mercies which he has lavished over nature, and is also so well fitted to regulate the conduct of the human race in the discharge of their various duties to God, to themselves, and society, that I cannot but hope the best effects from its promulgation in the present form." But, with all his admiration for the teaching of Christ, Râmmohun Roy would never become a convert to Christianity. When Dr. Mitford, the first Bishop of Calcutta, endeavoured to convert him, and in doing so

dwelt not only on the truth and excellence of the Christian religion, but spoke of the honour and repute that he would acquire as the first apostle of Christ in India, Râmmohun Roy felt so offended at the suspicion that he could be moved by such motives that he never called on the Bishop again. In all his discussions with missionaries and others, Râmmohun Roy took his stand on the Veda as the word of God, divinely inspired, and, therefore, infallible. It was on that foundation that he established the new Church which has since become famous under the name of Brâhma-Samâj, the Church of the believers in Brahman, the Supreme Spirit.

After he had built and endowed a house of prayer at Calcutta in 1830, he proceeded on his journey to England, being sent as Envoy by the Emperor of Delhi, and wishing himself to see what a Christian country really was. He was received with great distinction everywhere, and all who saw him spoke of him with the highest admiration. He also went to Paris, where he was received by Louis Philippe. After his return to England he went to pay a visit to Dr. Carpenter and other friends at Bristol, and there he died suddenly in September, 1833.

The lecture closed with an estimate of Râmmohun Roy's character. The Râjah was represented as an unselfish, honest, and bold man. Easy as it might seem to us to give up idolatry, it was a bold thing for a boy of sixteen to say, "I will not worship what my father worships; I will not pray as my mother prays. I will look out for a new God and new prayers, if haply I may find them." In after life he incurred the risk of the loss of his ancestral property, he was insulted, and even his life was threatened in the streets of Calcutta. In all these struggles he had nothing to support him but the Veda and the voice of his conscience, and a man who could fight so good a fight as he did, deserves to be ranked among the great benefactors of the human race. In conclusion the lecturer alluded to the latter growth of the Brâhma-Samâj, and more particularly to that momentous crisis when the Veda was deprived of its divine right, and the Brâhma-Samâj, under Debendranâth Tagore, became a Church without a Bible. It was shown that this important change was brought about by the influence of European scholarship on the minds of the prominent members of the Brâhma-Samâj. The mere fact of the Veda being printed and published in Europe, and thus being rendered accessible to every student, was sufficient to convince every unprejudiced mind that it was a venerable, but not a sacred, a human, but not a divine, book. To Râmmohun Roy the Veda was true, because it was divine; to Debendranâth Tagore it was divine, because it was true. It will have to be proved by the future history of the Brâhma-Samâj whether eternal truth requires always a miraculous halo, or whether she can rule human hearts, unadorned by priestly hands, clad only in her own simplicity, beauty, and majesty.

GENERAL MONTHLY MEETING,

Monday, February 4, 1884.

The DUKE OF NORTHUMBERLAND, LL.D. D.C.L. President, in the Chair.

The Earl Percy, M.P.
The Lord Sudeley,
The Rev. Edward Samuel Dewick, M.A. F.G.S.
Mrs. Charles Hawksley,
Sidney George Holland, Esq. LL.B.
William S. Playfair, M.D. F.R.C.P.
Augustine Robinson, Esq.
James Thorne, Esq.
Robert Younger, Esq. B.A.

were elected Members of the Royal Institution.

SIR JOSEPH HOOKER, K.C.S.I. C.B. F.R.S. &c. was elected a Manager in the room of the late Sir William Siemens, F.R.S.

The Managers have received the following letter :—

(*Translation.*)

DEAR SIR, BERLIN, 12*th December*, 1883.

I acknowledge with much gratitude your kind and sympathising note of the 3rd inst., as well as the Resolution passed at the Meeting of Managers of the Royal Institution of Great Britain in honour of my beloved brother William, whose memory will ever be dear to us.

The recognition of my brother's great worth, so warmly expressed in this Resolution, proves a real consolation to the bereaved sorrowing family of him who has been so prematurely taken away, and will henceforth form for them in itself a memorial of him. These tokens of grateful remembrance of services rendered, shown by the learned and scientific Societies of England, and the mourning honours which the cultivated classes of England, my brother's beloved adopted country, have paid him since his death, afford that great country at the same time a further title to fame, since they are a fresh proof that England values and honours merit without enquiring whence it originates.

I beg you, dear Sir, to convey to the Managers of the Royal Institution the heartfelt thanks of my brother's bereaved family for the Resolution sent to me.

With the expression of my high esteem,

I am, Sir,
Yours faithfully,
Dr. WERNER SIEMENS.

To Mr. W. Bowman,
Hon. Sec. of the Royal Institution
of Great Britain,
Albemarle Street,
London, W.

The Cordial Thanks of the Members were given to Lady Siemens for her gift of the "Selenium Eye," which formed part of the subject of the very interesting discourse delivered by the late Sir William Siemens before the Members on Friday, February 18, 1876.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Geological Survey of India: Palæontologia Indica: Series XIV. Vol. I. Part 4. 4to. 1883.
Records. Vol. XVI. Part 4. 8vo. 1883.
The Secretary of State for India—Synopsis of the Great Trigonometrical Survey of India. Vols. XIV. XV. and XVI. 4to. 1883.
Sketch of the Dynasties of Southern India. By R. Sewell. 4to. Madras, 1883.
New Zealand Government—Statistics of the Colony of New Zealand, 1882. fol. 1883.
The Government of France—Documents Inédits sur l'Histoire de France: Dictionnaire Topographique du Department du Calvados. Par C. Hippeau. 4to. Paris, 1883.
The Meteorological Office—Hourly Readings, Part 1, 1882. 4to. 1883.
Sunshine Records, 1881. 8vo. 1883.
Report of the International Meteorological Committee, Copenhagen, 1882. 8vo. 1883.
Abney, Captain W. de W. R.E. F.R.S. M.R.I. (the Translator)—The Chemical Effect of the Spectrum. By J. M. Eder. 8vo. 1883.
Accademia dei Lincei, Reale, Roma—Atti, Serie Terza: Transunti. Vol. VII. Fasc. 16; Vol. VIII. Fasc. 1. 4to. 1883–4.
Asiatic Society, Royal—Journal, Vol. XVI. Part 1. 8vo. 1884.
Asiatic Society of Bengal—Proceedings, Nos. 7, 8. 8vo. 1883.
Astronomical Society, Royal—Monthly Notices, Vol. XLIV. Nos. 1, 2. 8vo. 1883.
Memoirs, Vol. XLVII. 1882–3. 4to. 1883.
Banker's, Institute of—Journal, Vol. IV. Part 10. 8vo. 1883.
Bavarian Academy of Sciences, Royal—Abhandlungen, Band XIV. 3te Abtheilung. 4to. 1883.
Methoden in der Botanischen Systematik. Von L. Radlkofer. 4to. 1883.
Birkett, John, Esq. F.R.C.S. F.L.S. M.R.I.—Le Nouveau Conducteur à Paris. Par F. M. Marchant. 16mo. Paris, 1823.
British Architects, Royal Institute of—Proceedings, 1883–4, Nos. 4, 5, 6. 4to.
Cambridge Philosophical Society—Transactions, Vol. XIII. Part 3. 4to. 1883.
Proceedings, Vol. IV. Part 6. 8vo. 1883.
Cambridge University Press, The Syndics—Mathematical and Physical Papers. By G. G. Stokes. Vol. II. 8vo. 1883.
Chamberlin, T. C. Esq. (the Compiler)—The Geology of Wisconsin. Vols. I. and IV. 8vo. 1883.
Chemical Society—Journal for Dec. 1883 and Jan. 1884. 8vo.
Clinical Society—Transactions, Vol. XVI. 8vo. 1883.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Series II. Vol. III. Part 6. 8vo. 1883.
Dax: Société de Borda—Bulletins, 2e Serie Huitième Année: Trimestre 4. 8vo. 1883.
East India Association—Journal, Vol. XV. No. 7. 8vo. 1883.
Editors—American Journal of Science for Dec. 1883 and Jan. 1884. 8vo.
Analyst for Dec. 1883 and Jan. 1884. 8vo.
Athenæum for Dec. 1883 and Jan. 1884. 4to.
Chemical News for Dec. 1883 and Jan. 1884. 4to.
Engineer for Dec. 1883 and Jan. 1884. fol.
Horological Journal for Dec. 1883 and Jan. 1884. 8vo.
Iron for Dec. 1883 and Jan. 1884. 4to.
Nature for Dec. 1883 and Jan. 1884. 4to.

Revue Scientifique and Revue Politique et Littéraire for Dec. 1883 and Jan. 1884. 4to.
Steamship for Dec. 1883 and Jan. 1884. fol.
Telegraphic Journal for Dec. 1883 and Jan. 1884. 8vo.
Franklin Institute—Journal, Nos. 696, 697. 8vo. 1883-4.
Geographical Society, Royal—Proceedings, New Series, Vol. V. Nos. 11, 12; Vol. VI. No. 1. 8vo. 1883-4.
Geological Society—Abstracts of Proceedings, 1882-3, Nos. 442-444. 8vo.
Grosvenor Gallery Library, The Directors—Catalogue of Books and Music. 2 vols. 8vo. 1883.
Inner Temple, The Hon. Sec. of the Hon. Society of the—Masters of the Bench, 1450-1883; and Masters of the Temple, 1540-1883. [Not published.] 8vo. 1883.
Johns Hopkins University—American Journal of Philology, No. 15. 8vo. 1883.
University Circulars, No. 27. 4to. 1883.
Linnean Society—Proceedings, 1882-3. 8vo.
Transactions: Botany, Vol. II. Parts 5, 6; Zoology, Vol. II. Parts 7, 8, 9; Vol. III. Part 1. 4to. 1883.
Lisbon, Sociedade de Geographia—Bulletin, 4e Serie, Nos. 2, 3. 8vo. 1883.
Manchester Geological Society—Transactions, Vol. XVII. Parts 11, 12. 8vo. 1883-4.
Mechanical Engineers' Institution—Proceedings, No. 4. 8vo. 1883.
Meteorological Society—Quarterly Journal, No. 48. 8vo. 1883.
Meteorological Record, No. 10. 8vo. 1883.
Munk, Wm. M.D. F.S.A. (the Author)—Reintombment of the Remains of Dr. W. Harvey. 8vo. 1883.
Newton, A. V. Esq. (the Author)—Analysis of the Patent, Designs, and Trade Marks Act, 1883. 8vo.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XXXII. 8vo. 1883.
Numismatic Society—Chronicle and Journal, 1883, Part 3. 8vo.
Oliver, Westwood, Esq. (the Editor)—Science Monthly, Illustrated. Vol. I. No. 3. 8vo. 1883.
Peacock, R. A. Esq. C.E. F.G.S. (the Author)—Saturated Steam: the Motive Power in Volcanoes and Earthquakes. 8vo. 1882.
Pharmaceutical Society of Great Britain—Journal, Dec. 1883 and Jan. 1884. 8vo.
Calendar for 1884. 8vo.
Photographic Society—Journal, New Series, Vol. VIII. Nos. 2, 3. 8vo. 1883.
Plowden, W. C. Esq. (the Compiler)—Report of the Census for India, 17 Feb. 1881, and Tables. 3 vols. fol. 1883.
Reid, Clement, Esq. F.G.S. (the Author)—Geology of the Country around Cromer. 8vo. 1882.
Richardson, B. W. M.D. F.R.S. (the Author)—The Asclepiad. Vol. I. No. 1. 8vo. 1884.
Rio de Janeiro, Observatoire Imperiale—Bulletin, No. 9. fol. 1883.
Royal Society of London—Proceedings, Nos. 227, 228. 8vo. 1883.
Philosophical Transactions, Vol. CLXXIII. Parts 3 and 4; Vol. CLXXIV. Parts 1 and 2. 4to. 1883.
Scottish Society of Arts, Royal—Transactions, Vol. XI. Part 1. 8vo. 1883.
Society of Arts—Journal, Dec. 1883 and Jan. 1884. 8vo.
Society for Psychical Research—Proceedings, Vol. I. Part 4. 8vo. 1883.
Statistical Society—Journal, Vol. XLVI. Part 4. 8vo. 1883.
St. Bartholomew's Hospital—Reports, Vol. XIX. 8vo. 1883.
St. Pétersbourg Académie des Sciences—Mémoires, Tome XXXI. Nos. 5-8. 4to. 1883.
Bulletins, Vol. XXIX. No. 1; Vol. XXXVIII. No. 4. 4to. 1883.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1883: Heft 9, 10. 4to.
Victoria Institute—Journal, No. 67. 8vo. 1883.
Yorkshire Archæological and Topographical Association—Journal, Part 30. 8vo. 1883.

WEEKLY EVENING MEETING,

Friday, February 8, 1884.

Sir William Bowman, Bart. LL.D. F.R.S. Honorary Secretary and Vice-President, in the Chair.

George J. Romanes, Esq. M.A. LL.D. F.R.S.

The Darwinian Theory of Instinct.

(Abstract deferred.)

WEEKLY EVENING MEETING,

Friday, February 15, 1884.

Sir Frederick Bramwell, F.R.S. Manager and Vice-President, in the Chair.

Professor Thorpe, Ph.D. F.R.S.

The Chemical Work of Wöhler.

The lecturer began by reminding his audience of the brilliant eulogy of Liebig which Professor Hofmann pronounced in the theatre of the Royal Institution some nine years ago, and said that it seemed fitting that something should be said concerning one whose life-long association with Liebig has exercised an undying influence on the development of scientific thought. The names of Liebig and Wöhler are inseparably connected. No truer indication of the singular strength and beauty of their relations could be given than is contained in a letter from Liebig to Wöhler written on the last day of 1871.

"München, 31 *December*, 1871.

"Ich kann das Jahr nicht ablaufen lassen, ohne Dir noch ein Zeichen meiner Fortexistenz zu geben und die herzlichsten Wünsche für Dein und der Deinigen Wohl im neuen auszusprechen. Lange werden wir uns Glückwünsche zu neuen Jahren nicht mehr senden können, aber auch wenn wir todt und längst verwest sind, werden die Bande, die uns im Leben vereinigten, uns Beide in der Erinnerung der Menschen stets zusammenhalten als ein nicht haüfiges Beispiel von zwei Männern, die treu, ohne Neid und Missgunst in demselben Gebiete rangen und stritten und stets in Freundschaft eng verbunden blieben."

The father of Wöhler, August Anton, was formerly an equerry in the service of the Elector William II. of Hesse; his mother was connected by marriage with the minister of Eschersheim, a village near Frankfort, and it was in the minister's house that he first saw the light, on the 31st July, 1800. When quite a boy, the bent of his mind towards natural science was directed by Dr Buch, a retired physician who had devoted himself to the study of chemistry and physics; and it was in the kitchen of his patron's house that he prepared the then newly-discovered element selenium, of which an

account was afterwards sent by Dr. Buch to Gilbert's 'Annalen,' with Wöhler's name at the head of it. In his twentieth year he entered the University of Marburg as a student of medicine, but all his leisure time was devoted to chemical investigation. He discovered, without knowing that Davy had anticipated him, the intensely poisonous iodide of cyanogen; and in a little paper on cyanogen compounds, communicated for him by Dr. Buch to Gilbert's 'Annalen,' we have the first description of the remarkable behaviour of mercuric thiocyanate on heating, which has led to its use in the so-called "Pharaoh's serpents."

Wöhler, attracted by the fame of Leopold Gmelin, left Marburg for Heidelberg, and in the old cloisters which at that time constituted the University laboratory, he began the work on cyanic acid which some four or five years later culminated in his great discovery of the synthesis of urea. In 1823 he obtained his degree, and on Gmelin's advice abandoned medicine for chemistry. The masterly analytical skill of the great Swedish chemist Berzelius no less than his labours towards the development of chemical theory had, at this time, made him supreme among the chemists of Europe, and to Stockholm, therefore, Wöhler determined to go. He was warmly welcomed by the illustrious Scandinavian chemist, of whom he has left us an interesting sketch in his "Jugend-Erinnerungen eines Chemikers" (Ber. Deuts. Chem. Gesell., 1875). Whilst at Stockholm he discovered, among other products, some new compounds of tungsten, notably the monoxychloride and the tungsten sodium-bronze ($Na_2W_3O_9$) which twenty-five years later was introduced into the arts as a bronze powder.

After a couple of months spent in travel with Berzelius in company with the two Brongniarts, Wöhler, at the expiration of his year's stay in Sweden, returned to Germany and prepared to settle at Heidelberg as "privat docent." On the recommendation of Leopold von Buch, Poggendorff and Mitscherlich, he was, however, appointed to the teachership of chemistry in the newly founded Gewerbe Schule in Berlin. Wöhler was now twenty-five, and in possession of a laboratory which he could call his own. One of the many problems which he at this time attacked was the isolation of aluminium, which he succeeded in obtaining by the method which nearly twenty years afterwards was worked out on a manufacturing scale by Sainte-Claire Deville. Deville caused the first bar of the metal thus procured to be struck as a medal, with the image of Napoleon III. on the one side and the name of Wöhler with the date 1827 on the other, and presented it to the German chemist.

But of the twenty-two memoirs and papers which Poggendorff's 'Annalen' exhibits as the outcome of Wöhler's activity during his six years' stay in Berlin, that on the artificial formation of urea is by far the most important. Probably no single chemical discovery of this century has exercised so great an influence on the development of

liberal thought in science, and the words with which Wöhler closes his account of the molecular transformation of ammonium cyanate—a body of purely inorganic origin—into urea, a substance which of all that might be named is the most characteristic of the action of the so-called *vital force*, are full of meaning. "This unexpected result," he says, "is a remarkable fact in so far as it presents an example of the artificial formation of an organic body, and indeed one of animal origin out of inorganic materials."

At about the time that Wöhler made this great discovery he became acquainted with Liebig. When Wöhler was at Stockholm, thinking and working on cyanic acid, Liebig was in Paris engaged with Gay Lussac on the study of the metallic compounds of fulminic acid. The result of the investigations was to show that the explosive fulminic acid and the innocuous cyanic acid were of identical composition. The idea that bodies could exist of identical ultimate composition and yet possess essentially different properties was then new to science. Berzelius, the great chemical law-giver of his time, scouted the notion as absurd, until the discovery by his pupil Wöhler of the molecular transformation of ammonium cyanate into urea forced him to realise the fact, and to coin for us the word *isomerism* by which that fact is denoted. On the proposition of Wöhler, the two chemists engaged to work together. "Es muss wirklich ein böser Dämon sein," wrote Wöhler to Liebig, "der uns immer wieder unvermerkt mit unsern Arbeiten in Collision bringen und das chemische Publicum glauben machen will, wir suchten dergleichen Zankäpfel als Gegner absichtlich auf. Ich denke aber, es soll ihm nicht gelingen. Wenn Sie Lust dazu haben, so können wir uns den Spass machen, irgend eine chemische Arbeit gemeinschaftlich vorzunehmen, um das Resultat unter unserm gemeinschaftlichen Namen bekannt zu machen . . . Ich überlasse die Wahl des Gegenstandes ganz Ihnen."

Their first work in common was on mellitic acid. Shortly after its appearance, Wöhler and Liebig undertook a joint investigation on cyanuric acid, in the course of which they observed the extraordinary transformation of that acid into cyanic acid and the reconversion of the cyanic acid into cyanuric acid—one of the most remarkable instances of polymeric rearrangement known to the chemist.

In 1831 Wöhler was called from Berlin to Cassel, and for some little time he was wholly engaged in the planning and erection of his new laboratory at the Gewerbe Schule in that town. In the spring of the following year, he proposed to Liebig to attempt to clear up the confusion respecting the nature and relations of the essential oil of bitter almonds. This, as a piece of work, was perhaps their masterpiece. The investigation on the radicle of benzoic acid will ever remain one of the greatest achievements in the history of organic chemistry. It was full of facts, and rich in the promise of new material. The immediate effect of the paper was to establish the doctrine of organic radicles by demonstrating the existence of groups of bodies which had

their analogues and prototypes in inorganic chemistry. The concluding words of the memoir strike in fact the key-note of the whole investigation. "In once more reviewing and connecting together the relations described in this paper, we find that they may be grouped round a common nucleus which preserves intact its nature and composition in its associations with other bodies. This stability has induced us to regard this nucleus as a kind of compound element, and to propose for it the special name of benzoyl."

A significant feature of the memoir was that each of the substances described and correlated was the type of a group of bodies, some of which were known, but of which the analogies and relations were unthought of; others of these bodies had yet to be discovered, a matter of little difficulty when the modes of their origin had been indicated. The effect of this memoir on the chemical world was very great. Berzelius indeed regarded it as the dawn of a new day in vegetal chemistry, and proposed that the new radicle should be named *proin* (from πρωΐ, the beginning of day) or *orthrin* (from ὄρθρος, daybreak.

Wöhler remained in Cassel nearly five years. In the autumn of 1835 died Stromeyer, Professor of Chemistry in the University of Göttingen, and the choice as to his successor lay between Liebig and Wöhler. Eventually Wöhler was selected, and he entered on his work at Göttingen in the early part of 1836. He was succeeded at Cassel by Bunsen, who was at that time "privat docent" at Göttingen. In the October of 1836, Wöhler was ready for fresh work, and he proposed to Liebig to work out the singular origin of benzaldehyde (bitter almond oil) from amygdalin under the influence of a nitrogenised ferment, termed by Liebig and Wöhler, emulsin. Amygdalin in presence of water is decomposed into benzaldehyde, prussic acid, and sugar (glucose). Both the emulsin and the amygdalin exist together in the almonds, but are contained in separate cells and are only brought into contact by the rupture of the cell-walls and the solvent action of the water. Amygdalin was the prototype of a large and important group of substances classed together as the glucosides.

At the instigation of Wöhler the friends again returned to the question of the chemical nature of uric acid, and the memoir which they published on this subject is of the profoundest interest not only to the chemist but also to the physiologist. Uric acid, originally discovered by Scheele, was shown in 1815 by William Prout, then a boy of nineteen, to be the main constituent of the solid excreta of reptiles; other chemists had succeeded in obtaining various derivatives from it, indeed Prout himself had prepared from it the so-called purpuric acid—a substance which years afterwards, as murexide, obtained a transitory importance in the arts as a colouring matter. But nothing was known concerning the constitution of the body or of its relations to its derivatives until Wöhler and Liebig attacked the problem. The extraordinary mutability of uric acid, which had baffled and

deceived previous investigators, afforded to Wöhler and Liebig the clue to a labyrinth which led to a veritable treasure-house, and the marvellous insight and rare analytical faculty of these two great men were never more clearly indicated than in the way in which they trod this intricate maze. No fewer than fifteen new bodies were added to the list of chemical compounds, and these were correlated with the same masterly perspicacity that was so strikingly exhibited in the memoir on the radicle of benzoic acid. Some of the greatest triumphs of modern chemistry are seen in the synthesis of organic bodies; that organic chemistry was about to advance along this line was clearly foreseen by Wöhler and Liebig. In opening their account of this, the last great work they did in common, they say:—

"From this research the philosophy of chemistry will draw the conclusion that the ultimate synthetical formation in our laboratories of all organic bodies, in so far as they are not organised (in so weit sie nicht mehr dem Organismus angehören) may be regarded as not only probable but certain. Sugar, salicin, morphin, will be artificially obtained. As yet we know nothing of the way by which this result is to be attained, inasmuch as the proximate materials for forming these bodies are unknown; but we shall know them."

Henceforth the friends worked but little in common; Liebig's energies were spent in other directions, and Wöhler turned his attention chiefly to inorganic chemistry. In concert with Sainte-Claire Deville, he investigated boron and its compounds with aluminium and nitrogen. The readiness with which boron unites with nitrogen, and the mode in which the compound may be decomposed, led Wöhler to a conception of the origin of boric acid and borax in the volcanic waters in which they are frequently found. In collaboration with Buff, he discovered the spontaneously inflammable hydride of silicon, the analogue of marsh gas, and thereby laid the foundation-stone of a superstructure which in time to come may be only less imposing than that built up of the compounds of carbon. Many years ago Wollaston noted the presence of lustrous copper-coloured cubes in the slags from iron-blast furnaces which he assumed to be metallic titanium. Wöhler proved this substance to be a compound of carbon, nitrogen, and titanium, and showed how it might be obtained. Of all the elements known to the chemist up to the period of Wöhler's cessation from work, it may be safely affirmed that there was not one but that had passed through his hands in some form or other, and the number of minerals and meteorites he analysed is legion. In all, he was the author of 275 memoirs and papers; of these, fifteen were published with Liebig.

In philosophic contentment, happy in his work, in his home life and in his friendships, Wöhler lived out his four-score years and two. He made Göttingen famous as a school of chemistry; on the completion of the one-and-twentieth year of his connection with the

University, it was found that upwards of 8000 students had attended his lectures or worked in his laboratory. He was a man whom the world has delighted to honour, and there was hardly an academy of science or a learned society which has not in some way or other recognised his services to science. He was made a foreign member of the Royal Society in 1854, a corresponding member of the Berlin Academy in 1855, Foreign Associate of the Institute of France in 1864, and in 1872 he received the Copley medal from the Royal Society. He died on the 23rd September, 1882.

[T. E. T.]

WEEKLY EVENING MEETING,

Friday, February 22, 1884.

SIR WILLIAM BOWMAN, Bart. LL.D. F.R.S. Honorary Secretary and Vice-President, in the Chair.

SIR FREDERICK BRAMWELL, F.R.S. V.P. Inst.C.E. *M.R.I.*

London (below bridge) North and South Communication.

Two towns A and B, situated on the opposite banks of a tidal river. The current even at springs never great. The width never more than 440 yards. These towns have river frontages of some six and a half miles. Not inconsiderable towns, therefore—far from it; in fact, one of them has a population of 890,000, and the other a population of 655,000. Each, therefore, greater than Manchester, or Liverpool, or Birmingham, as large as Dublin or Glasgow, larger than Dresden, Milan, Rome, and other capitals or celebrated cities; and, taken together (the true way of considering them), more than half the size of Paris, larger than Berlin or than Vienna; yet, strange to relate, no means are provided by which a carriage can go from one town to the other, or, indeed, by which a foot passenger can (with one trifling exception) walk from one side of the river to the other, unless the passenger, or the driver of the carriage, is willing to journey from whatever part of the town he may be in, to the extreme west, where he will find a bridge. Moreover, the inhabitants of these two towns are not at war, for they are the subjects of one Sovereign,—in fact, they are practically members of one municipality; they speak the same language, thus there is no frontier custom-house, not even an *octroi*, and no need therefore to keep up a separation either for the prevention of invasion, or of smuggling, nor is intercourse limited by the necessity of an interpreter.

Where can these two towns be situated? Large as they are, must they not be the decaying remains of some two cities in the far east, Persia or China, from which all energy is absent? In the far east as understood by the world outside England they are not, but they are indeed cities in the far east, according to the views of most of the audience I have the honour of addressing to-night. The designations of their subdistricts and streets, although relieved at rare intervals by such pleasant names as Cherry Garden Stairs and Nightingale Lane, are, as a rule, barbarous and uncouth: Tooley Street, the home of the three tailors; Horselydown; Dockhead; Jacob's Island (see 'Oliver Twist'); Rotherhithe; Pickle Herring Stairs, on the one side;—East Smithfield; Ratcliff Highway; Wapping

(where the Claimant was born); Boarded Entry; Limehouse; and the Isle of Dogs on the other. Yet to attain this far east demands no passage by a P. and O. (Polite and Obliging) steamer, no traversing of a Suez Canal: a Hansom cab and 5*s.* will take you there in three-quarters of an hour.

I am afraid you will accuse me of a levity, unbecoming as well the importance of the subject as the gravity of the Royal Institution; but for years this condition of things, that the 890,000 inhabitants on the 16 square miles to the north-east could only communicate with the 655,000 on the 42 square miles to the south-east by going westward to London Bridge, has struck me as something perfectly ludicrous: a feeling that was intensified, when, not so long ago, I listened to the arguments and evidence laid before a Parliamentary Committee, grounded on the petty details of how many waggon loads a-day traversed the imperfect means of communication which now exist, put forward with the object of convincing that Committee that no better means were needed; that is to say—Afford no facilities for traffic, thereby keep it down to a minimum, and then argue that facilities are not needed for a minimum traffic. The development of this argument might be as follows:—Stop up Piccadilly from end to end for repairing, ascertain that only ten carts a-day came up side streets to the houses that could be reached therefrom, and then determine there was no need to be at the cost of completing the repairing, because the returns showed so small a traffic. You will say this is absurd. I agree, it is absurd, but it is no more absurd than the arguments which are used in reference to below-bridge communication. I venture to suggest that the true way to look at the question is the one which I have adopted to-night—two enormous towns, with practically no means of communication, and separated only by a puny stream, for puny it really is. I have on the wall a large map of London. I have temporarily covered over the part to the west of London Bridge, leaving visible the "Below Bridge" part only. Let us dismiss from our minds the north and south of London above that bridge, districts (or towns, as I shall call them hereafter) which really have nothing to do with the question before us, except, that I propose to refer to them directly, by way of illustration; and let us ask ourselves whether it is not absolutely incredible, and a matter which, when stated, inevitably appears to be ridiculous, that this condition of separation of town A, from town B, should exist?

Is such a state of things exceptional? To answer this question we will consider what has been done in other cases—New York and Brooklyn. The well-known map is on the wall. The 1,350,000 inhabitants on Manhattan Island (New York) are separated by the East River from the 585,000 inhabitants of Brooklyn; but for years past these two millions have made strenuous efforts to, as far as lay in their power, annul this separation. They have established numerous lines of steam ferries, starting many times in the

hour, making the passage in but a few minutes, and giving all passengers shelter, warmth, light, and the most scrupulous cleanliness, in a way that shames anything that we can show of a similar nature on this side of the water. Excellent as this ferry service is, and carrying as it does, 112,000 persons a day, it did not satisfy the needs of the inhabitants of these two towns, and the result has been, the construction of that magnificent engineering work, the East River High Level Bridge, opened on the 24th of last May; to a rough diagram of which I now point. Three suspension spans, the central one being 1595 feet, the two side spans 930 feet each, the bridge from abutment to abutment therefore, 3455 feet in length; the clear height above high water 135 feet; the width of the platform 85 feet, giving accommodation for a central footway, a double line of rails for a rope tramway, a double line of road for carriages. The foundation of one of the piers had to be carried down to a depth of 78 feet below high water, and was executed by the aid of compressed air (to Lord Cochrane's invention of which system reference will hereafter be made), the men working (at the maximum) in a pressure of 33 lbs. to the square inch above atmosphere. This bridge with its approaches, and the land for them, it is believed, has cost from 14 millions to 15 millions of dollars—say 2,800,000*l.* to 3,000,000*l.*, of which Brooklyn, the smaller town, contributes two-thirds, and New York one-third.

Having established this communication across the East River to Brooklyn, the inhabitants of New York are not satisfied. To the west of New York runs the Hudson, cutting it off from the State of New Jersey, and its thriving towns; and although there is maintained across this stream, a splendid service of ferry boats, there is now being driven below the Hudson (and again by the use of Lord Cochrane's system) a tunnel, of about a mile in length, to effect a communication that shall be independent of ice and fogs, and one through which the railway trains, that are now compelled to stop at the New Jersey shore, shall be able to enter the City of New York itself.

Another instance nearer home—Liverpool with its 552,000 inhabitants on the one side of the Mersey, and Birkenhead, and the neighbouring towns, on the other side. Here is established a system of steam ferries, which, so far as easy access from the shore to the boat, notwithstanding a great rise and fall of tide, is concerned, are not surpassed, even if indeed they be equalled, anywhere; and the boats themselves of late years have been good, though not up to the standard of the ferries of New York. But the inhabitants of Liverpool and Birkenhead have not been content, and, as you know, even since the beginning of this year, the preliminary drift way of the Mersey Tunnel has been completed from side to side. This tunnel (of which the engineers are Mr. Brunlees and Mr. Fox) is one mile long from shaft to shaft (1232 yards being under the river), and its floor at the lowest point will be 145 feet below the level of high water;

there is a minimum thickness of 30 feet of rock between its roof and the bed of the river. The tunnel will afford accommodation to two lines of railway, and there will be passenger stations close to the river bank, fitted with hydraulic hoists.

When on the 8th of January, 1884, the two parties who had advanced from the opposite shores, made the junction of their respective works, it was found that the error horizontally was covered by the width of the ranging pole, and vertically was only one-eighth of an inch.

But there are two towns nearer home that appear to appreciate the advantages of other communication, than that which can be afforded by ferries, or than can be obtained by a journey of several miles to a single bridge at one of their extremities. The towns to which I told you I should have to revert—those which lie north and south of the Thames to the west of London Bridge. One hundred and thirty years ago, when, from the best information I can obtain, the population west of London Bridge did not exceed, if indeed it amounted to, 600,000, including both north and south, this population found out that it was not convenient when it was desired to cross the river to be obliged to go eastward to London Bridge for that purpose, and thereupon were built, Westminster, opened in 1750, and Blackfriars, completed in 1770. These were public bridges free of toll. As London grew, companies were formed, who built successively Vauxhall Bridge, opened in 1816; Waterloo (originally called the Strand Bridge), opened in 1817; and Southwark in 1819; while the Lambeth Bridge, the Albert Bridge, and the Wandsworth Bridge, also provided by companies, and the Chelsea Bridge, built by the Government, have all been erected within the last few years, as have been the various railway bridges. I have omitted mention in this chronological list, of Battersea, and of Putney Bridges, because at the time they were built they were in fact rural, and not Metropolitan Bridges; but the growth of London has embraced them within its bounds. Both these bridges were the result of private enterprise. Putney Bridge, the really operative Act for which was not passed till 1728, deserves notice, because it is an instance of delay in giving sanction for a communication the need of which was felt more than half a century before the Act was passed: the proposition to build the bridge having been successfully resisted on absurd grounds (notably in 1671), that the erection of this Bridge would stop the growth of the prosperity of London, as the North and South Traffic would pass the Thames at Putney, and would no longer come through the Metropolis.

It is not so long ago since I had the honour of lecturing in this room, not upon the means of communication from side to side of the Thames, but upon those from side to side of the English Channel. The arguments which were used before the Committee of last year, which sat upon the Channel Tunnel, must, I think (judging from a pamphlet I hold in my hand, but for the authenticity of which I do not vouch), have been taken from those used in reference to this

Bridge in 1671, and without the proper acknowledgment of the source from whence they were derived. Bear with me while I give you a portion of the speech of Mr. Jones, one of the then members for London, as I wish you to have the benefit of his own views expressed in his own emphatic language.

"Mr. Speaker,—It is impossible to contemplate, without feelings of the most afflictive nature, the probable success of the Bill now before the House. I am sensible, that I can hardly do justice by any words of mine, to the apprehensions, which not only I myself personally feel upon the vital question, but to those which are felt by every individual in the kingdom, who has given this very important subject, the smallest share of his consideration. I am free to say, sir, and I say it with the greater freedom, because I know that the erection of a bridge over the river Thames at Putney, will not only injure the great and important city, which I have the honour to represent, not only jeopardise it, not only destroy its correspondence and commerce, but actually annihilate it altogether. (Hear, hear.) I repeat it in all possible seriousness, that it will question the very existence of the Metropolis; and I have no hesitation in declaring that, next to pulling down the whole borough of Southwark, nothing can destroy London more certainly than building this proposed bridge at Putney. (Hear, hear.) Allow me, sir, to ask, and I do so with the more confidence because the answer is evident and clear, how will London be supplied with fuel, with grain, or with hay, if this bridge is built? All the correspondences westward will be at one blow destroyed. . . ."

I wish time would admit of my quoting the speech of Sir Henry Herbert to the like effect.

Upon a division, the Bill was thrown out by a majority of 13, 54 voting for the Bill and 67 against it.

I fear this Putney Bridge digression has withdrawn our minds from the fact that our two towns were well supplied with bridges, some free and some the subject of tolls. But their inhabitants were not, however, content even with this condition of bridge accommodation: they determined to get rid of the tolls; and Southwark first, and the various other bridges more recently, have been purchased and thrown open for the use of these inhabitants, who in the seven and a quarter miles from London Bridge to Putney, have ten road bridges and one foot bridge, while, as has been said, the two eastern towns in practically the same number of miles, London Bridge to Blackwall, have no means of communication whatever, except the Thames Tunnel, which is now used for a railway, and a foot subway, at Tower Hill, of only 6 feet 6 inches clear internal diameter.

As long ago as 1796, a tunnel was suggested to connect Gravesend with Tilbury, but it is believed very little work was done. In 1804, a tunnel, called in the Act of Parliament an Archway, was commenced from Rotherhithe to Limehouse. By 1809 the very small (5 feet by 3 feet) preliminary driftway for this tunnel was executed

for about 1050 feet from the Surrey shore, and was then abandoned in consequence of the inflow of water, and the want of funds.

In 1826 the talented Marc Isambard Brunel commenced, from a point a little to the east of Rotherhithe Church, the celebrated Thames Tunnel. I have had lent to me by Mr. Law, who was an assistant engineer in the carrying out of the Thames Tunnel, a diagram of the work, as intended. It was proposed, that on each side of the river there should be sunk a circular shaft 200 feet in diameter, and containing a spiral roadway, having a rise of one in twenty-five, by which vehicular traffic could ascend and descend. At the bottom the roads were to pass across two other shafts, each of 50 feet diameter, provided with staircases, for the foot passengers. The tunnel itself had two separated roadways, with footways, and arches of communication, from the one roadway to the other. There is no doubt that this was a very wonderful work, and one that largely occupied public attention. Forty years ago the particulars of its construction were well known, and it would have been a waste of time to have described them to an audience in this room; but now the details are forgotten, and I think that I may be forgiven if I ask attention for a few minutes, while I relate how the work was carried out.

The ground on the Surrey side having been levelled, a wooden curb provided on its exterior with a cast-iron ring, having a cutting edge, was laid upon the levelled ground, and on a framework of short piles to temporarily support it, and upon this curb was built, in the form of a tower, the brick lining, that was to be, of the shaft. The outside of the tower was smoothly covered with Roman cement, the material employed in the brickwork itself. As soon as the tower had attained some 20 feet in height, the earth was excavated from within the ring, which then commenced under the weight of the brickwork to cut its way into the earth, and, with the tower, to descend into the excavation. As the tower sank, more and more brickwork was added to the top, and, as need arose, the walls were weighted. In this way the curb was got down to within 40 feet of the required distance, when it stuck fast, and the remainder of the work had to be done by the process of underpinning. The shaft being thus completed down to the required level, and the pumping-machinery having been fixed, an opening was formed in that side of the brick tower which was towards the river, and the driving of the excavation was commenced. This excavation was a perfect rectangle, 38 feet wide by 22 feet high, filled with brickwork in Roman cement, but of course leaving the two holes constituting the double roadway. The work was carried out by means of the "Shield."

Through the kindness of the Council of Trinity College, Dublin, I have been enabled to place on the wall enlarged diagrams of the tunnel. The diagram shows that the shield consisted of twelve vertical frames, placed side-by-side like books upon a book-shelf. Each of these frames extended the full height of the excava-

tion, and was divided into three storeys, so as to afford in the whole shield working spaces for thirty-six men: in some cases it appears that, under a dangerous state of things, as many as four men were needed in one of these spaces, which were technically called "boxes." Each frame was in fact a girder, the top and bottom of which were supported by powerful screws, abutting against the end of the already completed brickwork of the tunnel, while the weight was taken by a screw-leg, the foot being stepped into a shoe, carried on a bottom plate, resting on 3-inch elm planks, which were supported on the soil. Radius bars were provided between the frames, so that while one frame could be moved in advance of its neighbours, it was prevented by the radius bars from rubbing against them, and thus the friction was diminished. The tops of the frames supported cast-iron plates, which at the end next the brickwork were continued by wrought-iron plates, that extended over it so as to exclude the earth. Plates of a generally similar construction were carried by the sides of the shield. Each frame supported at its front a number of horizontal boards about 6 inches deep and about 3 inches thick; these were called poling-boards; they were latched one to another, so as to make a kind of flexible wooden blind. Screws at the ends of the poling-boards, supported them from their own frame, except at the times when the frame was being pushed forward, and then the supporting screws, abutted on the frames adjoining their own. Assume, for example, the frames to be numbered from 1 on the left hand to 12 on the right, and that numbers 1, 3, 5, &c., were three inches more forward than numbers 2, 4, 6, &c.: the workmen in these even-numbered frames, took down one poling-board in each box, picked out the earth, which he found in front of it, for a distance of 6 inches, then replaced the board, took down another board, and repeated the operation, until the earth in front of these even-numbered frames, had been excavated the 6 inches. Then (their poling-boards being, as already said, supported by the screws abutting on the neighbouring, the odd-numbered, frames) the even-numbered frames were screwed forward the 6 inches, that had been excavated, and thus became 3 inches in advance, of the odd-numbered frames. The odd-numbered frames were then dealt with in the manner that had been pursued with the even-numbered, and their poling-boards being supported from the even-numbered frames, the odd numbers were driven on the 6 inches, so as once more to become the advanced frames. As the shield went forward the brickwork was added to, so as to be always about 9 feet behind the medium position of the face of the poling-boards, and in this way the sheet-iron tailpieces of the roof and sides were kept supported by the brickwork. The arches were turned on narrow centerings, which travelled forward as the work progressed.

The openings in the intermediate wall between the two roadways were not formed in this wall as it was built, but were cut through afterwards. I have described the operations in the shield as going on

very systematically and quite comfortably, but this was by no means the case in practice. The first 550 feet of the tunnel, it is true, were driven without much difficulty, and were finished in about sixteen and a half months from its commencement on January 1st, 1826, giving an average rate of progress of from 7 to 8 feet per week, and for many weeks as much as 11 or even 12 feet of advance were made; but on the 18th of May, 1827, the material through which the tunnel was passing being little better than semi-liquid slush, and the then shield not being of the same excellent construction as the second one (the one I have described), the water broke in and compelled the stoppage of the works. Clay was lowered in bags, from above through the water, into the hole formed in the bed of the river, and by September 28th of the same year, forward work was resumed. On January 12th, 1828, the water again broke in. By May 6th the workmen once more mastered the water, but shortly after, the funds failing, the ends were temporarily walled up, a length of 740 feet having been driven, and this condition of affairs remained until 1835, when, the Government having agreed to advance money, a new shield was made, and the work was resumed.

The Wapping shaft was prepared and sunk, in readiness for a junction with the advancing tunnel, and in 1843, on the 25th of March, the tunnel was opened for foot passengers. The whole length, from the side of the Rotherhithe shaft to the side of the Wapping shaft, is 1208 feet, of which 1014 feet are under the river at high water. It was stated by Mr. Brunel, that while the total weight of material excavated was 63,000 tons, that of the brickwork put in its place was only 26,000 tons, thus showing, that so far from an extra strain having been imposed upon the soil, it had been relieved of 37,000 tons of load. The Wapping shaft was got down the whole way by the sinking process, and no underpinning was needed. I saw this shaft in course of execution, and Mr. Brunel took me into the shield when it was about 130 feet from the Wapping shore; and I remember that the blows of the pile-engines, which were driving piles, for the tunnel wharf, were distinctly audible, although some 200 feet of earth intervened at that time.

The large shafts for the vehicular traffic were never sunk, and the tunnel, as you all know, is now part of the East London Railway, affording a communication from the Liverpool Street Station in the City, to New Cross, and from thence to Brighton, and to other places.

I cannot quit the subject of the Thames Tunnel, without alluding to an important invention, to which it gave rise. In 1830, you will remember, the ends of the tunnel were walled up, and no work was being carried on. In that year the celebrated Lord Cochrane (whose locomotive I helped to make when I was an apprentice) had a grant of a patent, No. 6018, for a method which he said was applicable to the making of such a tunnel "as that now executing beneath the River Thames at Rotherhithe," and to the sinking of cylinders for bridge foundations. This invention consisted in the application of air, com-

pressed to such a point as to entirely, or partially, balance the pressure of the water, in the soil through which the work was being carried out, and thus to stop, or to greatly diminish, its influx. Lord Cochrane showed the means of doing this—of passing men in and out of the work, by air locks—of passing materials in and out by the same means, or by tubes having their ends open but sealed by a column of water; in fact, he described all that has ever since been found necessary, or even desirable, in compressed-air cylinder sinking; but, so far as I know, no use was made of the invention until 1850, when it was employed in sinking the cylinders for Rochester Bridge. I felt I ought not to let this admirable invention, of so very remarkable a man, pass without a brief notice.

The only other executed work, making communication from side to side of the river, below bridge, is that already referred to—the small subway from Tower Hill. This consists of a cast-iron tube, about 7 feet external diameter, and about 6 feet 6 inches clear internal diameter. The engineer was Mr. P. W. Barlow. It was opened for traffic in the beginning of the year 1871, the work being executed in stiff clay the whole way, no difficulties of importance were met with. The authorised capital is 26,000*l*. It was originally intended to use some mechanical means of transport, but this idea was given up. The toll is a halfpenny, and the persons using it are said to number 1,000,000 per annum, or an average of about 3000 per day: a very fair piece of evidence, in favour of the needs of increased communication. In the year 1877, a steam ferry for vehicular purposes was established, just over the Thames Tunnel, and was opened on the 31st October of that year. The rise and fall of tide were allowed for by counterbalanced platforms, raised and lowered by means of chains, connected to horizontal hydraulic presses. This ferry worked for a short time, but a ship ran into the framework of the platform of the Surrey side, and shattered it, and for some time past the ferry has ceased to be used.

With respect to projects: a suggestion that has been made on more than one occasion is to construct a duplex bridge. This is a renewal of a very old plan proposed as far back as the beginning of the century by Colonel Bentham and also by Mr. Dance, the architect, whose bridge, however, was practically a double one. Each of these gentlemen intended his bridge as a substitute for old London Bridge, and proposed to construct it in this manner to allow seagoing vessels to come up the river as far as Blackfriars.

The diagrams on the walls are enlarged from the drawings in the Report of a Parliamentary Committee on this subject, which sat, off and on, for about five years, at the end of the last century, and at the beginning of the present. The intention was to have the bridge double for a short distance, in one case, and for its whole length in the other; the distance between the two sections being sufficient to accommodate a vessel, so that if it were coming up stream it should pass by an opening in the eastern section of the bridge, the road traffic on that section being stopped, and then the traffic on the other

section being stopped, the movable part of that section should be opened, and the ship should be allowed to come through.

A Bill for a similar project was deposited in the present session of Parliament by a private company.

In the Report of the Parliamentary Committee to which I have referred, is also to be found Telford's proposition for a one-arch cast-iron bridge, to replace London bridge. A diagram of it is on the wall.

The Parliamentary Committee to whom this and other schemes were referred took the advice of the scientific men of the day on the subject; among them was John Playfair, Professor of Mathematics at Edinburgh College, who winds up his report of the 27th April, 1801, upon Telford's single-arch bridge, with words of modesty and wisdom, which I think are well worthy of being repeated and of being borne in mind:

"I cannot, however, make an end of this report without observing to the Committee that it is not from theoretical men that the most valuable information in such a case as the present is to be expected. When a mechanical combination becomes in a certain degree complicated, it baffles the efforts of the geometer and refuses to submit even to his most improved methods of investigation. This holds particularly of bridges, where the principles of mechanics, aided by all the resources of the higher geometry, have not yet gone farther than to determine the equilibrium of a set of smooth wedges, acting on one another by pressure only, and in such circumstances as, except in a philosophical experiment, can hardly ever be realised. It is therefore from men bred in the school of daily practice and experience, and who, to a knowledge of general principles, have added, from the habits of their profession, a certain *feeling* of the justness or insufficiency of any mechanical contrivance, that the soundest opinion on a matter of this kind is to be obtained."

Time will not admit, nor do I think you would be interested, were I to call your attention in the most cursory manner (even by a list of names) to all the various schemes that have, from the days of the Thames Tunnel up to this date, been proposed for below-bridge crossings, but I will at once mention some of the latest. These are—the High Level Bridge, proposed by the Metropolitan Board of Works, to span the river from Little Tower Hill (the road immediately to the east of the Tower) to Horselydown Old Stairs; the Low Level Bridge at the same spot, with opening spans for the passage of vessels, advocated by the Corporation; and the Bill of last year promoted by a private Company, for the making of a subway parallel with the small subway I have already mentioned as extending from Great Tower Hill, on the west side of the Tower.

The question forces itself upon one—If, as appears self-evident, some means of communication below bridge is needful, and has been needful for the last fifty years, why has it not been made before this? The answer is, the difficulties of suitable access, the difficulties of the

work, and above all, as regards certain modes of crossing, the magnitude of the interests affected, or alleged to be affected. With respect to the difficulty of access. The map shows that on the northern side of the river, first a portion of the bank is occupied by the Tower, to the east of which is Little Tower Hill, already mentioned (the point from which so many projectors have proposed to start their bridge or tunnel), then the bank is occupied by the St. Katherine's Docks, and these are separated by nothing but a roadway (Nightingale Lane) from the London Docks, which extend down the river until Shadwell is reached: that is to say, from the western side of the Tower downwards there are only two places where, for a length of about one and three-quarter miles, there is not a barrier to the approach to the river; these two places are, Little Tower Hill and Nightingale Lane.

After the eastern end of the London Docks is passed on the north side, the south is found to be occupied by the Surrey Canal Docks and the Commercial Docks down to Deptford, and moreover, on the north the Limehouse Basin of the Regent's Canal, which Basin is in truth a collier dock, cuts off ready communication. These difficulties of approach apply to bridges and to tunnels. With bridges the difficulties are practically insuperable, as the approaches must needs extend across the docks, cutting them in halves. It is possible, no doubt, to carry a tunnel under the docks, as has been done in the case of the extension of the Thames Tunnel, to make a communication with the East London Railway, but the work is very expensive, and, worst of all, the underground distance is most materially increased.

With respect to bridges, another class of difficulties, not connected with want of access, arises. If a fixed bridge having only such an elevation above the water as that of London Bridge be adopted, i. e. if a non-opening low-level bridge be resorted to, then obviously this bridge becomes the head of the masted shipping navigation, as London Bridge is at the present time, and its erection would result either in a diminution of the space available in the Port of London, or else, in the gradual extension of its business lower down the river. If this, the more probable result, took place, then obviously it must give rise in time to the repetition of the present difficulty, viz. the existence on the banks of two large towns, to the east of the bridge forming the head of the port, without any means of communication from side to side of the river, which separates them. Moreover, the sum demanded as compensation for existing interests, between the new bridge and London Bridge, would be something that as regards its total would have to be stated in millions, and as regards its increments, for every few extra yards of distance between the two bridges, by thousands. On the ground of first cost, therefore, the temptation would be great to place the new bridge as near to London Bridge as possible, notwithstanding that other considerations would urge that it should be at a reasonable distance below it. If it be suggested, as it has been over and over again, that these difficulties could be met by making certain spans of the bridge to open, the answer is, that in a tidal

river, there would be great danger of vessels which intended to pass through, drifting against a closed bridge unless some provision were made to have their approach announced by telegraph, as no doubt might be done, and at the same time the bridge were opened in anticipation, so as to ensure a clear passage for the vessel when she did arrive. No argument founded on the ease with which vessels can be got in and out of docks (generally at high water and always in the absence of appreciable current in the passage through which they pass) would be valid, as applied to bridge openings in a tideway. Moreover, experience teaches, that the Parliamentary opposition of the persons interested in the traffic above the suggested low-level opening bridge, would be as strenuous, or even more strenuous, than in the case of a non-opening low-level bridge, as in the latter instance these persons would know, that if the bridge were built, they must be fully compensated for that which would be undoubted damage, while they would feel that if an opening bridge were to be made, an arbitrator might believe these openings would suffice, might think their apprehensions idle, and might either refuse them compensation altogether, or give them one of but very small extent. That a Parliamentary Committee would be strongly influenced by such opposition, is no mere speculation. In the year 1879, when the Metropolitan Board of Works bridge, to which I have already alluded, was considered by such a committee, the persons occupying wharves above its site, objected that if vessels were compelled to strike their topmasts, or even their topgallant masts, to pass under the roadway of that bridge, which it was intended to place so as to give a clear headway of 65 feet above Trinity high water, or as much as 85 feet above low water, it would form a ground of objection on the part of shipowners and captains to take freights for the wharves above the bridge, and thus their competitors below the bridge would be placed in an unfairly favourable position; and although it was shown, that the great bulk of the shipping which came above the site of the intended bridge, could pass, without striking anything more than their topgallant masts, or without striking masts at all, the opposition of the wharfingers prevailed, and the bill was thrown out. A diagram of the bridge is on the wall, and a model of the bridge and of the neighbouring districts, with an enlarged model of the circular approach, 300 feet in diameter, and having a spiral road with an inclination of 1 in 40, is on the table before you.

From a wharfinger's point of view, it appears to me that a duplex bridge, such as proposed in 1801, would not remove the difficulty, but on the contrary, would add to it, as there would be two bridges to pass instead of one, doubling the dangers attendant upon the passage of one bridge.

But the other question remains to be considered: supposing the wharfingers satisfied, would the public who had to use the bridge be satisfied? Would it be tolerated that the traffic across a single

bridge, should be interrupted at unknown times, to admit of the passage of vessels? and if the duplex bridge were resorted to, matters would be but little improved. It is of the very essence of the success of a means of important communication, that there should be perfect certainty of passing at all times.

Two other modes of crossing from side to side deserve notice, as instances of ingenious contrivance: one, the dredging of a channel across the river, the bed of which channel is to be perfectly horizontal, and is to support several lines of rails. On these rails a subaqueous carriage is to be placed, having a framework tall enough to carry a platform at the height of the level of the shore. A steam engine on the platform, by appropriate connection makes the wheels revolve, and thus the carriage travels from side to side of the river; such a contrivance is in use at St. Malo, and a drawing of one is upon the wall. It need hardly be said, however, that, having regard to the danger to the rails from ships' anchors, and from the chances of vessels grounding on them, this cannot be looked upon as a feasible plan to be employed in the Thames. The next scheme is the converse of the St. Malo plan, viz. a platform at the shore level suspended by a framing from an overhead girder placed at such height as to be clear of the tallest masts. It will be seen that both these plans result in a platform moving like the deck of a steamer across the river, but, unlike the deck of a steamer, not affected by changes in the water-level.

We now come to the question, What plans are really open for consideration, bearing in mind that the object to be attained is the maximum of accommodation, coupled with the minimum of interference with other interests, and (most closely allied to this second consideration) the minimum of cost? In arriving at the cost, there has to be borne in mind, the acquisition of property, the expenses of the work, and the annual charges for maintenance or for working, as the case may be. In my view the possible plans resolve themselves into three: high-level bridge—a tunnel—ferries.

Leaving ferries on one side for the moment, as being rather very valuable supplementary means, than principal means, the choice really lies between a high-level bridge and a tunnel.

I have stated the objections that were successfully urged by wharfingers and shipowners, against the Metropolitan Board high-level bridge proposed in 1879. It is clear that nothing short of raising the bridge so high that no masts, not even the topgallant masts, need be struck, would put an end to the opposition. The question may be asked, Why not make the bridge of this height? and this brings into consideration the difficulties of the work. I do not mean to say that, in the present state of engineering knowledge, a bridge at a level high enough for the Monument to stand under, could not be built with the most absolute certainty as regards safety, and success as a structure, if money enough were spent upon it. Let me instance the magnificent work of the Forth Bridge, with its 1700 feet spans

and its clear headway of 150 feet above high water. Thanks to the kindness of Messrs. Fowler and Baker, the engineers, I am enabled to show you a diagram of the bridge, and to give a clear conception of the vast dimensions of this work, there are drawn on the same sheet the Menai, the St. Louis, and some other well-known large-span bridges. So far as the efficiency of the structure is concerned, it is a simple question of money, but a bridge high up in the air, although it may satisfy those who pass under, will be of no use to those who wish to pass over, if it cannot be readily reached from the ground level. Although on the north the ground at Tower Hill rises some 31 feet above high water, the shore on the south is practically on a level with it, and thus on that side the whole height from high water to the level of the bridge would have to be climbed.

Formerly, no mode would have been open, to get the traffic on and off such a bridge, other than inclined roads. These, for heavy traffic ascending, could not have been made successfully at a greater inclination than 1 in 30, better still 1 in 40. To give an idea of what appearance is presented by certain gradients, it may be said in passing that St. James' Street varies from a maximum gradient of 1 in 23 to a minimum of 1 in 60, and has a mean of 1 in 32; while the eastern extremity of the Thames Embankment, where it joins the road leading on to Blackfriars Bridge, is made with a regular rise of 1 in 40. The upper surface of the floor of the bridge could not be less, to avoid all fancied interference with shipping, than 150 feet above Trinity high water; an inclination of even 1 in 30 would demand therefore a length of 4500 feet for the Surrey approach, while 1 in 40 would obviously require as much as 6000 feet. In these days, however, it is perfectly feasible to raise and lower the whole traffic of such a bridge by hydraulic power; but when the large number of vehicles which, as I shall show you presently, it may fairly be anticipated would pass, come to be dealt with, the space demanded for double groups of lifts, the ascending and the descending (for there are almost insuperable difficulties in making one set of lifts answer both for the ascent and the descent), and the cost of the establishment of these lifts, render the abandoning of inclines, for certain portions of the traffic, very undesirable.

And although no doubt, as I have said, such a high-level bridge could be constructed and worked, no estimate has been made of it; the cost, however, of a bridge with 100 feet clear headway has been got out, and, including acquisition of property, amounted to as much as 745,000*l.*, while the annual charges would probably be 18,000*l.* If no other mode were available, then the importance of the matter is so great that, large as these payments are, they would be more than justified, and the money would be well laid out; but, before determining to adopt such a plan, it is proper that the alternative of a tunnel should be investigated. For many years the difficulties met with in the execution of the Thames Tunnel, brought subaqueous tunnelling into disfavour; but there are more ways than

one of making a tunnel, or rather of making a covered way, be it under the land or under the water. Moreover, engineering appliances have become developed, I need hardly say, in the sixty years since the Thames Tunnel was commenced, and better materials exist at cheap rates. Brunel had a shield made of cast iron; then steel would have been an impossible metal for the purpose—it was sold not by the ton, but by the pound; in these days the shield would be made of steel, at no greater cost than was then needed for one in cast iron. Screwjacks, demanding a great expenditure of manual power to be exerted in a confined space, would now be replaced by hydraulic presses worked by a steam-engine on the surface; and the pumping of the water which made its way in at the shield, instead of demanding the labour of forty men, costing for the day and night work 150*l.* a week, could be effected with ease by an hydraulic engine, by a compressed-air engine, or even by electricity, which latter agent would also afford the means of illumination without heat or contamination of the air.

These would be the means by which (even if the covered way were truly a tunnel—that is to say, a "burrowing through the earth") such a work could be carried out with a cheapness, an expedition, and a certainty that were not possible sixty years ago. But, improved as the means of tunnelling are, it is still desirable, that there should be some considerable thickness of earth, between the bed of the river, and the top of the hole, that is being tunnelled. Therefore, if the process of tunnelling be followed, it is seen there would have to be allowed first, the depth of the river, next the minimum thickness of earth over the excavation, and then the depth, from the top of the excavation to the surface of the roadway. If the minimum thickness were employed, that ought to be allowed, the result would be, with the height of roadway it is proposed to give in the tunnel I am about to mention to you, to put its floor 80 feet below Trinity high water.

Now we have considered the question that we must have an ascent to reach from the ground to the roadway of a high-level bridge, it is equally clear we must have a descent to extend from the surface of the ground down to the roadway of a tunnel; and if that roadway were 150 feet below the surface of the ground, the difficulties of the descent would obviously be as great as the difficulties of the ascent. But as has already been said, even if tunnelling be resorted to, the depth of the roadway below high water need be no more than 80 feet; but by following a process in making this subaqueous road which is pursued in corresponding cases on land, the depth can be so much reduced, as to be only 60 feet. I mean the process technically known as "cut and cover." The name expresses quite clearly what is done, and tells us that the excavation is cut down from the surface, and that then (the brickwork and covering arch being put in), earth is filled over the top, and the surface is restored. This plan is one that can be perfectly well followed for a subaqueous road, and can be carried out in the following manner:—A cofferdam of a certain

length is raised to a few feet above high water, the water is pumped out, the bed of the river is exposed, the earth is excavated down to the required depth, and the concrete put in, and the brickwork of the tunnel including its covering arch is completed. The cofferdam is then prolonged for another length, which is similarly treated, leaving, it will be seen, the front end of the original cofferdam as a mere partition in the middle of a longer cofferdam pumped dry on both sides of this partition. Under these circumstances it is easy to make the partition tight against the executed work. Then the upper part of the sides, and the hinder end of the first length of dam, can be removed, and with the requisite new parts, be formed into another length, in front of the portion being executed, and thus step by step the work can be carried across the river with absolute certainty. The remark may be made, supposing bad ground is met with, why won't the whole structure sink ? Bad ground means in this case quicksand full of water, and under this condition of things there would be no pressure, forcing the tunnel to sink, greater than that which the bad ground had already borne ; for, as seen in the case of the Thames Tunnel, after allowing for the openings, the tunnel as a whole is lighter than the earth, which it replaces. With respect to the nature of the cofferdam, it may be urged by those acquainted with these subjects, that the depth is too great for piling, but piling is not the only mode of making a cofferdam.

Sir Joseph Bazalgette, for that portion of the Victoria Embankment between Westminster and some one hundred yards to the east of Waterloo Bridge, employed a succession of oval iron caissons, which were sunk in a continuous line and were united by grooves and tongues. These caissons have the advantage of being sinkable, to any needed depth, by internal excavation, and of forming by their lower parts an admirable protection to the completed work, while the upper parts are readily unbolted by the aid of a diver, and are used over and over again, in the successive stages of the cofferdam.

There is another mode, however, by which, without any cofferdam ordinarily so called, cut-and-cover work can be executed, and that is by employing an enormous diving-bell, and using compressed air. Some years ago Mr. Fowler and Mr. Baker proposed an application of this system for the Humber Tunnel. A sketch of the apparatus is shown upon this diagram.

An open bottom vessel some 160 feet long, by 42 feet wide, by 12 feet deep, immediately below, and made in one with an air-tight vessel of exactly equal size, forming a float. On the top of this last-named vessel there was a framework 45 feet high, carrying a working deck 160 feet long by 55 feet wide, on which was to be placed the whole of the machinery. The framework supported a certain number of screw piles on each side. The apparatus was to be used in the following manner :—It was to be towed over the place where the tunnel was to be made, and then, the machinery being set to work, the screw piles were to be driven into the earth on each side of the vessel ;

water was then to be admitted into the float so that the vessel would gradually sink, until the edges of its open bottom cut their way into the soil; then air was to be driven into the open vessel, expelling the water, and enabling the workmen to descend through shafts provided with air locks, and to carry on the work of excavation within this chamber of 160 feet long, 42 feet wide, and 12 feet deep. As the excavation was continued, the vessel sank lower and lower, until the desired depth was attained, the sinking being guided by the screw piles. At this, the lowest point, the working stage was still sufficiently raised above high water. When the lowest point was reached, the work of building the tunnel proper was begun, and was carried on till the lower half was finished, thereby nearly filling the chamber to the top. Then the water was pumped out of the float, allowing the whole apparatus to rise to a certain distance, admitting the water to flow over the lower part of the finished work, but permitting this to be added to; and in this way it is possible to build a tunnel of some 23 feet total depth by the aid of an air chamber of but little more than half that depth. The 160 feet length having been finished, the float was entirely emptied of water so as to come up to the surface, the screw piles were withdrawn, and the apparatus was moved forward to make the next length of the tunnel. There was an extremely ingenious provision by which the junctions of the successive lengths were effected, but time will not admit of my going into that detail.

Last year Mr. Law, whom we have already had to thank this evening for the diagram of the Tunnel, exhibited to a Parliamentary Committee the model which he has kindly lent me to-night, showing how "cut-and-cover" could be executed by a compressed-air apparatus. This model represents the manner in which it is intended to construct, under the Thames, the tunnel of the Charing Cross and Whitehall Electric Railway, and here also for the purpose of saving depth "cut-and-cover" is resorted to. The tunnel has its sides and roof made of wrought iron, lined with concrete. On a staging prepared in the river, a 50-feet length of the tunnel is riveted up and lined, and then from this staging, it is lowered on to the bed of the river by hydraulic apparatus; the ends of the length are first temporarily closed, but the bottom is left open, and thus the length becomes a diving-bell; air is pumped into it, and air shafts and air locks being provided, men descend and carry out the excavation. As this is done the length sinks further and further, until the desired depth is reached; recesses in the ends of the length contain flexible tubes, into which a water pressure can be put, so as to expand the tubes, and thus make a provisional joint against the length already completed. The temporary closing of the end is then removed, and the permanent attachment of the one length to the other is made.

In any one of these ways the tunnel may be made, but when made, people have a sort of feeling that after all a tunnel is an ill-lighted, ill-ventilated, damp, unpleasant sort of a place. There is not the slightest reason why this should be; and I will undertake to

say it would not be. Having regard to the avoidance of interference with existing interests, to the cost, and to the diminished length of the approaches, it does appear that the tunnel is preferable to a high-level bridge. I told you a little time back that I would for the moment dismiss the question of ferries, and I ought then to have given a few particulars connected with the traffic, to show that something more was needed for the principal means of communication. Let me supply that omission now. In the year 1882 careful observations were taken, and it was found on a Monday in the month of August, that in twenty-four hours there passed over London Bridge 22,795 vehicles and 117,451 foot passengers. It need hardly be said that these did not traverse the bridge at a uniform rate, throughout the twenty-four hours. The table on the wall * shows the allocation of the traffic to the various hours, giving a minimum of 7 vehicles and of 130 foot passengers per hour between two and four in the morning, and a maximum of 1846 vehicles per hour between the hours of 10 A.M. and noon, and a maximum of 13,037 foot passengers between the hours of 8 A.M. and 9 A.M., while the average per hour between the hours of 8 A.M. and 8 P.M. is as much as 1578 vehicles and 7644 foot passengers. Assuming that the new mode of communication were only to get a fraction of this traffic, and that the total traffic was not increased, even then it would not be well to attempt to pass it by ferry boats traversing a crowded river, but there can be no question that the existence of a new communication will largely develop traffic. Notwithstanding these 22,795 vehicles over London Bridge, 3554 were found to pass over Southwark, only 500 yards above London Bridge, and with its very bad gradient on the Middlesex side, while 13,288 were found to pass over Blackfriars, only 1288 yards above London Bridge. Moreover, the freeing of the toll bridges may be cited in support of this development of traffic by increased accommodation. Before the toll was removed, there passed over Waterloo, Charing Cross, Lambeth, Vauxhall, Chelsea, the Albert, Battersea, and Putney Bridges, during 24 hours, 46,960 foot passengers and 8077 vehicles. Four to five years after the toll was removed, there passed over the former toll bridges I have enumerated 114,760 foot passengers per day, and 22,621 vehicles, being practically two and a half times the number of foot passengers, and two and three-quarter times the number of vehicles. Part of this increase was no doubt due to the natural growth of London in the five years, but that part must have been but a very small proportion of the whole.

With respect, however, to this increase in the population, the table † on the wall shows you (to go back for fifty years) that in 1831 there were within that which is now the Metropolitan area a total population of 1,655,200, divided into—north-west of London Bridge,

* See Table A and Diagram A′ in Appendix.

† See Table B in Appendix.

829,900; south-west of London Bridge, 237,000; giving for the western section of London, therefore, 1,066,900; north-east of London Bridge, 415,300; south-east of London Bridge, 173,000; giving for the below-bridge towns a population of 588,300. In 1881, half a century later, the total population within the Metropolitan area was 3,834,360, the north-west section 1,662,060, the south-west 627,300, making for the western towns 2,289,360; while the north-east was 890,000, the south-east was 655,000, making for the eastern towns a population of 1,545,000. Thus, while the increase in the whole Metropolitan area had been 2·31 times; west of London Bridge it had been only 2·14 times, while east of London Bridge it had been as much as 2·62 times; and in those fifty years new Blackfriars Bridge and new Westminster Bridge had been built at the public expense; Chelsea Bridge had been built at the public expense; Hungerford, Lambeth, the Albert Bridge, and Wandsworth Bridge had been built by companies, but had been subsequently acquired by public money; while the condition of communication, or rather of want of communication, to the east of London Bridge had remained as it was. Moreover, the rateable value is shown by another table * to have been in 1881 for the whole of London in which the abovementioned population lived, the sum of 27,970,552*l.*, divided as before into north-west 15,819,758*l.*, south-west 3,665,229*l.*, making a total for the west of 19,484,987*l.*; north-east 5,224,924*l.*, south-east 3,260,641*l.*, making a total for the eastern district of 8,485,565*l.*

Leaving London Bridge out of consideration, as this is common to the east and to the west, the western section has had provided for it, out of the public funds in the first instance, old Blackfriars, 261,500*l.*, new Blackfriars, 396,000*l.*; old Westminster, 400,000*l.*, and new Westminster, 393,000*l.*; and has acquired by purchase Southwark, paid for by the Corporation of the City at a cost of 384,000*l.*, and the various other bridges paid for by the Metropolitan Board at a cost of 1,332,025*l.*, to which has to be added the cost of new Putney, new Battersea, and new Hammersmith bridges, the underpinning of Waterloo, and the repairs to other bridges, 848,304*l.*: making a total that has been paid or will have to be paid for the bridge accommodation west of London Bridge of 4,014,829*l.*, or, taking the population of that district, of 1*l.* 15*s.* per head. While the only money, so far as I know, which has been contributed by the public purse to afford means of communication below bridge has been the sum for the completion of the Thames Tunnel—I believe, but am not certain, about 200,000*l.*, or, taking the population of that district, only 2*s.* 7*d.* per head.

I trust that after all these dry but necessary figures, you will think I have said enough, to show that a communication is needed; and I will now state very briefly and generally the nature of the work of which the Metropolitan Board, by their engineer, Sir Joseph

* See Table C in Appendix.

Bazalgette, have this session deposited plans, for giving a tunnel communication. It is proposed that this should be made between the Hermitage Basin of the London Docks on the north side, and Mill Stairs and St. Saviour's Dock on the south side.

The access on the north side will be from East Smithfield by a widening of Nightingale Lane, situated as I have already said between the St. Katherine and the London Docks. On the large map is shown the position of the approaches. At this point there is excellent communication to Tower Hill and the Minories northward; and by Trinity Square, and the New Street above the District railway, westward; Dock Street and Leman Street afford access to the Commercial Road and Mile End Road; while Ratcliff Highway, leads down to Shadwell and Limehouse. The ascent from the tunnel will be made by an open cutting in the middle of New Nightingale Lane, and on each side of this there will be roads at the present level of the surface, affording access to the St. Katherine Docks on the west, to the London Docks on the east, and on the south to the widened street of Wapping. On the Surrey side the ascent will be forked, terminating westerly in New Tooley Street, and easterly in Jamaica Road, affording excellent communication with Bermondsey, Rotherhithe, and Deptford. It is proposed to have a single archway 36 feet wide, having two 4-feet footpaths, for carters to lead their horses, and a clear roadway therefore of 28 feet, with a headway of 17 feet in the centre. Parallel with this and forming part of the work, there will be a passage for the foot traffic, kept separate from the roadway. This will be 14 feet high, and 12 feet wide. The roadways will descend to the deepest part of the tunnel by an inclination of 1 in 26 on the Surrey side, and 1 in 25 on the Middlesex side; but near to each shore a lateral roadway will be provided which will have a rise of only 1 in 40, and this roadway will lead to a group of hydraulic lifts, by which the whole of the heavy traffic will be raised, these lateral roadways being on the respective "near" sides. By this arrangement, while all the traffic will go down the inclines, and the light traffic—such as cabs, carriages, tax-carts, and matters of that kind—will go up the inclines (the steepest of which is at a much less pitch than parts of Ludgate Hill, or, as has already been stated, parts of St. James's Street), the heavy waggons will avoid these inclines, drawing off to the lifts, and will be taken to the surface without any exertion or hindrance, thus rendering the passage through the tunnel far easier for such traffic than that over London Bridge; as to pass this bridge a waggon, however heavily laden, coming from Tooley Street, has to make an ascent of as much as 33 feet, while it would traverse the tunnel and find itself upon the high ground of East Smithfield without having had to do any climbing whatever.

Provision of lifts will be made at once, for taking up all the heavy traffic, assuming it to be one-half of that which now passes over London Bridge at the busiest hour of the day. The approach to the

tunnel on both sides being open, as already said, to the sky, the covered part will not be more than 1200 feet long; a person walking three miles an hour will pass through this in less than five minutes; but having regard to the fact that it will be lined with white tiles, will be lighted by incandescent lamps, will be kept thoroughly clean, and will be well ventilated, no one need hurry with the desire of getting out of an unpleasant place. The roadway of 28 feet wide is sufficient for four vehicles abreast, and thus quick and slow traffic can both be accommodated. It is estimated that the total cost of this work including acquisition of property, embracing that taken from the Docks, will not exceed the sum of 1,800,000*l.* The estimate being one put forward by the Engineer of the Metropolitan Board, is entitled to the confidence of the public, as that Board can point to a large number of works executed within the estimated amounts.

I had put on one side for a time, the question of ferries. I will now say a few words upon them. It is proposed to at once establish two: one on the site of the old horse ferry, from the Isle of Dogs to Greenwich, the other from Woolwich to North Woolwich.

These ferry services will be conducted by steamboats probably propelled by paddles, and having deck accommodation for carriages and horses, and cabin accommodation for foot passengers. I have said probably propelled, because there is always the alternative of the floating bridge arrangement, where chains are laid in the bed of the river, and being hauled in over properly shaped wheels, the bridge is drawn along as is practised at Portsmouth, Southampton, and many other places. With respect to the difficulty occasioned by the variation in the height of the water, this may be met in a variety of ways.

If a floating bridge be used, its rising and falling gang-board effects a connection at all times of the tide with a sloping "hard." If steamboats be employed, then there are the following modes available:—

1. By the vessel coming alongside a float connected to the shore by hinged bridges, as at all our steamboat piers.

2. By the float being raised vertically by hydraulic power to the required level.

3. By the deck of the vessel being lifted the whole height, as is done in Stephenson's floating bridges on the Nile.

4. With a view to the diminishing of the angle made by the hinged bridge at extreme low water, a combination of No. 1 with No. 3. By any of these modes thoroughly satisfactory results can be obtained.

In conclusion, I trust I have caused the audience present here to-night to wonder how it is that the two towns A and B have been so long cut off the one from the other by a stream which never attains to a quarter of a mile wide, to agree with me that this is a state of things which should no longer be suffered to continue, and also to agree with me that in common honesty we, at the West End of London, are bound to do our best to ensure that those at the East shall obtain the much-needed accommodation.

APPENDIX.

TABLE A.

NUMBER of FOOT PASSENGERS and VEHICLES passing over London Bridge in the 24 hours commencing at 6 A.M. on Monday, the 14th August, 1882, and ending at 6 A.M. on Tuesday, the 15th August, 1882.

Total number of Foot Passengers in the 24 hours 117,451
Total number of Vehicles in the 24 hours.. .. 22,795

	Foot Passengers.		Vehicles.	
	Percentage of Total.	Number.	Percentage of Total.	Number.
6 to 7 A.M.	2·8	3,289	0·02	5
7 ,, 8 ,,	4·7	5,520	2·8	638
8 ,, 9 ,,	11·1	13,037	5·4	1,231
9 ,, 10 ,,	10·9	12,802	7·6	1,733
10 ,, 11 ,,	5·5	6,460	8·1	1,846
11 ,, 12 ,,	5·2	6,107	8·1	1,846
12 ,, 1 P.M.	4·1	4,815	7·6	1,732
1 ,, 2 ,,	2·9	3,406	5·5	1,254
2 ,, 3 ,,	4·	4,698	6·8	1,550
3 ,, 4 ,,	4·	4,698	6·8	1,550
4 ,, 5 ,,	6·2	7,282	7·6	1,732
5 ,, 6 ,,	7·7	9,044	7·4	1,687
6 ,, 7 ,,	9·	10,570	6·8	1,550
7 ,, 8 ,,	7·5	8,809	5·4	1,231
8 ,, 9 ,,	4·9	5,755	3·8	866
9 ,, 10 ,,	3·1	3,641	2·8	638
10 ,, 11 ,,	1·9	2,232	2·1	479
11 ,, 12 ,,	0·91	1,069	1·3	296
12 ,, 1 A.M.	0·41	482	0·8	182
1 ,, 2 ,,	0·22	258	0·04	9
2 ,, 3 ,,	0·18	212	0·03	7
3 ,, 4 ,,	0·11	129	0·04	9
4 ,, 5 ,,	0·31	364	0·06	14
5 ,, 6 ,,	2·1	2,466	1·1	251
	99·74	117,145	97·99	22,336
Add Error in percentages	·26	306	2·01	459
	100·00	117,451	100·00	22,795

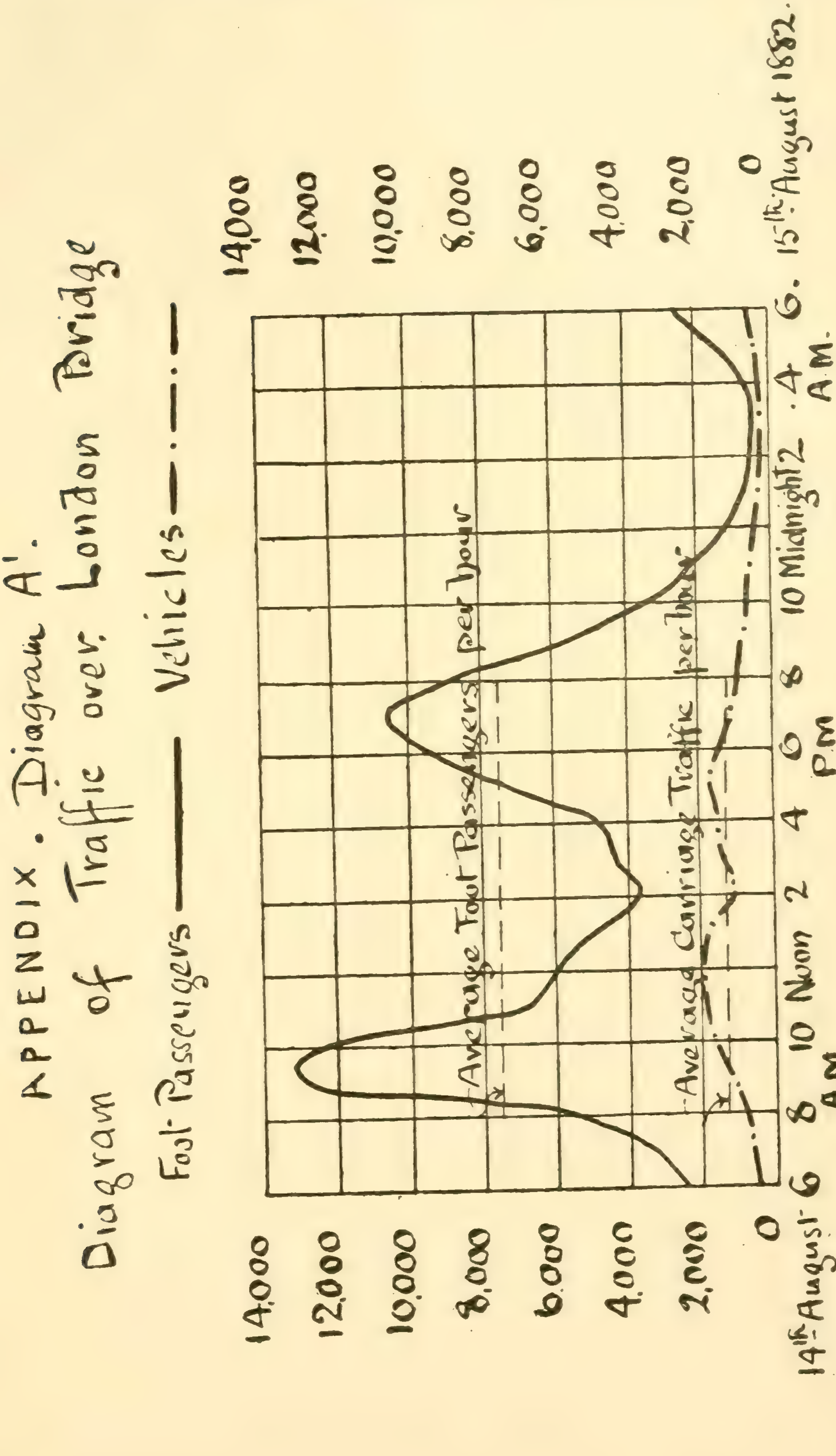

APPENDIX. Diagram A'.
Diagram of Traffic over London Bridge
Foot Passengers ——— Vehicles —·—·—
Average Foot Passengers per hour
Average Carriage Traffic per hour
14,000
12,000
10,000
8,000
6,000
4,000
2,000
0
6 8 10 Noon 2 4 6 8 10 Midnight 2 4 6.
A.M. P.M. A.M.
14th August
15th August 1882.

TABLE B.

POPULATION WITHIN METROPOLITAN AREA.

	West.		East.		
A.D.	North-West.	South-West.	North-East.	South-East.	Total.
1831	829,900	237,000	415,300	173,000	
	1,066,900		588,300		1,655,200
1881	1,662,060	627,300	890,000	655,000	
	2,289,360		1,545,000		3,834,360

TABLE C.

Rateable Value within Metropolitan Area.

	West.		East.		
A.D.	North-West.	South-West.	North-East.	South-East.	Total.
1881	£15,819,758	£3,665,229	£5,224,924	£3,260,641	
	£19,484,987		£8,485,565		£27,970,552

INDEX TO VOLUME X.

END OF VOL. X.

LONDON: PRINTED BY WM. CLOWES AND SONS, LIMITED, STAMFORD STREET AND CHARING CROSS.